UNANTICIPATED MONEY GROWTH, "TIME-TO-BUILD," AND PERSISTENCE UNDER RATIONAL EXPECTATIONS

By

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This work is dedicated to my parents.

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TABLE OF CONTENTS

page

ACKNOWLEDGMENTS..... iv

ABSTRACT..... vii

CHAPTERS

1 INTRODUCTION..... 1

2 LITERATURE REVIEW..... 8

Rational Expectations, Policy Neutrality, and Business Cycles..... 8
Barro's Approach to Testing the Policy-Neutrality Proposition..... 15
Reconciling Policy Neutrality with Persistence: Three Influential Propagation Mechanisms..... 36
The Barro Empirical Procedure: Technical Issues. 69

3 HYPOTHESES AND EMPIRICAL TESTING..... 90

Implications of the Propagation Mechanisms..... 91
Testable Hypotheses..... 148

4 EMPIRICAL RESULTS..... 174

Introduction..... 174
Preliminary Issues..... 175
Specification of the Nominal-Demand Forecasting Equations..... 184
Results Stemming from the Disaggregation of Real GNP..... 210
Further Results Stemming from the Disaggregation of Real GNP: Blinder-Fischer versus Time-To-Build (Hypotheses 4 and 5)..... 288
Tests of Time-To-Build Using Independent Survey Data on Production Periods (and Related Tests)..... 312
Analysis of Decisions to Start Multiperiod Investment Projects: Hypothesis 9..... 332
Results Stemming from the Disaggregation of Real Inventory Stocks..... 356

Analysis of the Explanatory Power of the Wage-Stickiness Propogation Mechanism: Hypothesis 13..... 384

5 SUMMARY AND CONCLUSIONS..... 413

Summary..... 413
Conclusions and Suggestions for Future Research. 427

APPENDICES

A DATA SOURCES..... 434

B GENERATION OF QUARTERLY PROGRESS PATTERNS FOR NONRESIDENTIAL AND RESIDENTIAL CONSTRUCTION.. 440

REFERENCES..... 448

BIOGRAPHICAL SKETCH..... 457

Abstract of Dissertation Presented to the Graduate School of the University of Florida in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy

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An important question in current macroeconomics is the extent to which the Rational Expectations approach to monetary theory is consistent with the widespread presence of persistence in macroeconomic time-series data. This study is an empirical investigation of one such possible explanation of persistence: the time-to-build propagation mechanism associated with the names of Kydland and Prescott. Time-to-build is tested not only against the data but also directly against its main competitors, the inventories-based explanation of Blinder and Fischer, and the wage-stickiness explanation of Fischer. Testing is carried out with U.S. postwar data using variants of Barro's "unanticipated money growth" technique, where a real response to lagged nominal shocks is taken as evidence of persistence. A number of the resulting findings are favorable to time-to-build. GNP-persistence largely is confined to the investment accounts, and disaggregation of investment reveals substantial persistence in

producers' durable expenditures and short-term persistence in residential structures. The data support the time-to-build hypothesis that the pattern of GNP-persistence is explained by the persistence of fixed investment over the alternative inventories-based hypothesis that that pattern is explained by the persistence of changes in inventories. There is strong evidence that construction progress patterns for nonresidential structures can explain the persistence of producers' durable expenditures, and some types of investor decisions to start multiperiod capital-construction projects ("starts") do not exhibit persistence. There is evidence that time-to-build accounts for the persistence of manufacturers' inventory stocks (while the Blinder-Fischer mechanism apparently accounts for the persistence of retailers' stocks). Finally, there is little evidence that the wage-stickiness explanation of persistence can account for the persistence of GNP in the postwar data. Other findings are not consistent with time-to-build: specifically, relatively little persistence is exhibited by nonresidential structures, there is no evidence that construction progress patterns can account for the persistence of structures, and certain types of "starts"--particularly those of nonresidential structures--exhibit too much persistence to be consistent with time-to-build.

CHAPTER 1
INTRODUCTION

An important implication of the Rational Expectations approach to monetary theory is that changes in the money supply can affect real variables such as the level of output or the unemployment rate only to the extent that such changes are unanticipated. In a world characterized by Rational Expectations, rational individuals will observe a policy rule being followed by the monetary authority and will take steps to insulate themselves from any systematic and predictable changes in the money supply which occur. Only to the extent that rational decisionmakers are unaware of such purely nominal changes can these changes have their desired impacts on the real values of macroeconomic variables.

Among several counterarguments advanced against the "policy-neutrality" position outlined above, one of the more durable has been the claim that it is inconsistent with the historical record of the United States and other relatively free-market economies. One might consider, for example, the impressive body of empirical evidence assembled by Friedman and Schwartz (1963), which documents the close relationship existing between changes in the money supply and in the level of real economic activity in the U.S. over the period 1867-1960. Such evidence strongly suggests that, in seeking to account for those serially-correlated

deviations in real output from trend which define the "business cycle," money-supply changes should be given a prominent role.

Yet the existence of a money-induced business cycle can be argued to be inconsistent with the Rational Expectations view, via a two-step argument. First, the proponent of the Rational Expectations approach to macroeconomics logically must take the position that, if the evidence of Friedman and Schwartz is to be reconciled with a monetary theory of the business cycle, then unanticipated rather than anticipated movements in money ultimately must be responsible for generating the cycle. Second, given this first point, it is unclear that a theory of the business cycle based on unanticipated money movements can be constructed which does not clash with the logic of the Rational Expectations hypothesis. Supposing, for example, that an unanticipated decline in money growth occurs in period t, there is no controversy over the assertion that such a change could generate a period-t rise in unemployment (and a fall in real output) to a level considerably above (below) its natural rate. What is not immediately clear is how such a monetary "shock" in t could raise unemployment above (lower output below) its natural rate for several successive periods. That is, it is unclear how such a theory could account for a <u>persistent</u> impact of money shocks, where such shocks generate serially-correlated movements by unemployment and real output over time.

The essential problem is that it is difficult to argue that a shock occurring in t would continue to be unobserved in subsequent periods. On Rational Expectations premises,

individuals have strong incentives to correct their past mistakes in forecasting key macroeconomic variables, and, at least in the case of the money supply, there is no reason to believe that individuals would be prevented from observing past actual values of monetary growth. Thus rational decisionmakers would have the means to quickly correct their past forecasting errors and in future periods would observe the true nature of the monetary shock that occurred in t. Under these circumstances, why should rational individuals make a real response in some period t+i to what is now a fully recognized shock from period t? Such a real response apparently clashes with the logic of the Rational Expectations hypothesis.

A problem analogous to the above arises in the context of the well-known empirical work of Barro (1977b, 1978, 1981b), which tests the relative explanatory powers of unanticipated versus actual money growth in unemployment and real-output equations. Barro interprets his findings as supporting the hypothesis that monetary change affects the real sector only to the extent that such change is unanticipated. In reaching this conclusion, however, Barro employs lagged values of unanticipated money growth as explanatory variables: The use of such lagged shocks in this context is crucial to the derivation of his results. Again it can be argued that, under Rational Expectations, such lagged shocks should not have real effects on current-period real variables, so that on these grounds Barro's evidence cannot be taken as favoring the "policy-neutrality" hypothesis.

Thus, on both of the above grounds, a reconciliation of persistence with the Rational Expectations hypothesis is crucial to the Rational Expectations approach to monetary theory. In seeking to meet the objections outlined above, advocates of the Rational Expectations approach traditionally have emphasized the distinction between, on the one hand, the forces ultimately responsible for the cycle, and on the other hand, the process by which money (or, more generally, nominal-demand) shocks are propagated onto the real sector. The counterargument starts with the observation that the economy's condition depends not only on money-supply changes but also on additional variables which, taken as a group, define the structure or "state" of the economy. Some of these "state" variables might reasonably be taken to behave in such a way that their values in period t affect the values of other real variables in subsequent periods. Examples might be the number of long-term investment projects started in period t, or the stock of inventories in t relative to that stock's long-run equilibrium value, or the nature of the long-term labor contracts negotiated in t. While it would be a contradiction of the Rational Expectations hypothesis for a money shock in period t to <u>directly</u> affect the values of real variables in future periods, such a contradiction would not exist if a period-t shock were to affect the value of one or more of these "state" variables in t which then, in turn, were to affect the future values of real variables. On this interpretation the impact of a period-t money shock on future real variables would be <u>indirect</u>, operating through some "propagation mechanism" in a

way which would not contradict the logic of the Rational Expectations hypothesis. The implication of this view is that, if the unobserved "state" variables responsible for the process of propagation could be identified and their impact taken fully into account, then unanticipated movements in money would be seen to directly have only a contemporaneous impact on real variables.

On the above grounds, the tendency for past nominal-demand shocks to affect current values of real macroeconomic variables might be reconcilable with the Rational Expectations approach to macroeconomics. A full reconciliation, however, requires that a persuasive case be made for the existence of some propagation mechanism of the type alluded to above, capable of translating short-term shocks into longer-term movements in real output, unemployment, and other such variables. Theoretically this requirement would appear to have been met in principle, as a number of models of the propagation process have been developed which impose both the assumptions of Rational Expectations and of a single-period information lag, but which nevertheless exhibit a current-period real response of output and unemployment to past shocks.

However, little empirical testing of these mechanisms has been carried out. In the absence of such testing, the actual capacity of the various propagation mechanisms to empirically account for the persistent response of real variables to nominal shocks remains unknown, despite the theoretical appeal of such mechanisms. Accordingly, in the continued absence of such empirical research, the consistency with the historical record of

the Rational Expectations approach to monetary theory remains unclear.

This study is an extensive empirical investigation of one prominent propagation mechanism: the "time-to-build" mechanism of Kydland and Prescott (1982) (adjusted to emphasize the potential impact of nominal-demand shocks). The time-to-build hypothesis asserts that a positive nominal-demand shock generates a relatively rapid start of construction of complex capital projects, which take a substantial amount of time to complete. Once started, these projects are typically completed despite the later knowledge that a purely nominal change triggered the start of the projects. The result is a flow of spending which continues for some time after the date of the shock; further, as these capital projects are completed, there is the possibility of additional production of goods in general due to the existence of more productive capital than there would have been in the absence of the shock. The result is the propagation of the initial stimulative effects of the nominal-demand shock forward into future periods, despite the existence of Rational Expectations, and despite a relatively rapid rate of information dissemination. The reverse effects occur in the event of a negative shock: Fewer projects are started in the period of the shock, bringing about less spending and production in future periods.

Empirical testing of the time-to-build propagation mechanism is carried out within the framework of Barro's "unanticipated money growth" reduced-form technique, for both annual and quarterly U.S. post-World War II data, using a variety of

nominal-demand-shock measures. In formulating testable hypotheses, the basic methods of approach are two: first, to disaggregate Gross National Product ("GNP") by type of product and investigate the persistence of the components of GNP under various conditions; and, second, to investigate the persistence of various macroeconomic variables closely related to but not actually part of GNP (such as inventory stocks and decisions by firms to start multiperiod construction projects). In developing and carrying out empirical testing of the time-to-build mechanism, two main classes of tests are utilized. First, the question is assessed of whether the predictions of the time-to-build mechanism are consistent with the historical record--whether the predictions of that mechanism are statistically rejected by the data. Second, the explanatory power of the time-to-build mechanism is directly tested against the explanatory power of two of the leading alternative propagation mechanisms which have been advanced in the literature; specifically, the "inventories-based" mechanism of Blinder and Fischer (1981), and the "wage-stickiness" mechanism of Fischer (1977a).

Chapter 2 presents a detailed review of the theoretical and empirical grounds underlying this study. Chapter 3 develops 13 hypotheses from the logic of the three propagation mechanisms reviewed in Chapter 2, and develops the general means by which these hypotheses can be tested. Chapter 4 presents the results of the empirical investigation. Chapter 5 is a summary and conclusion.

CHAPTER 2
LITERATURE REVIEW

Rational Expectations, Policy Neutrality, and Business Cycles

The questions of whether market-based economies are inherently afflicted by periodic intervals of prosperity and depression, and of whether government should play a prominent interventionist role in an attempt to "stabilize" these economies, have been sources of recurring controversy in the history of economic thought. The macroeconomic revolution ushered in by Keynes (1936), which answered both of these questions in the affirmative, led to a span of over three decades during which orthodox theory was founded on the premise that, at least in principle, an activist government policy invariably could achieve results certainly as desirable as, and usually superior to, those achievable through adherence to a government policy of macroeconomic "laissez faire." Critics of this view in general questioned the practicality of stabilization policy due either to the existence of long and variable effectiveness lags or to political constraints on policy, but, at least through the mid-1960s, the theoretical supremacy of the stabilization principle went essentially unchallenged in orthodox macroeconomics.

The countermovement of the late 1960s emphasizing expectations-formation in a macroeconomic context was begun by

Friedman (1968), who argued that activist monetary policy ultimately was not merely impractical but actually self-defeating. Friedman's analysis began by assuming that the monetary authority attempted to stimulate the real sector by permanently increasing the rate of growth of the money supply. The success of such a policy would depend on its ability to lower interest rates and/or exploit an allegedly negatively-sloped Phillips curve, possibilities requiring widespread money-illusion on the part of utility-maximizing individuals. However, money-illusion could not continue indefinitely, since utility-maximizing individuals would have strong incentives to avoid losing utility due to ill-informed decisions, and since the information allowing individuals to correct for the money-illusion--the true nature of the monetary policy--would be readily and cheaply available (this last point regarding information acquisition was not explicitly stated by Friedman but was clearly implied by his exposition). Friedman's argument therefore came down to the following two points. First, only to the extent that the monetary authority could conduct its policy in ways unperceived by the market--that is, in ways creating widespread money-illusion--could such policy have its desired effects. Second, standard neoclassical principles of utility maximization implied that the monetary authority's capacity to systematically generate money-illusion was limited. Friedman's key insight was that rational decisionmaking by individuals imposed meaningful limits on the monetary authority's ability to

manipulate the real sector, a key step towards the policy-neutrality arguments of the Rational Expectations school.

Friedman's argument, which emphasized that the case for systematic monetary policy was sensitive to assumptions regarding the behavior of inflationary expectations, heightened interest in attempts to understand the behavior of expectations in general. Speculation along these lines led to increasing interest in the work of Muth (1961), which had advanced the notion of "Rational Expectations," holding that expectations "are essentially the same as the predictions of the relevant economic theory," and asserting that "the economy generally does not waste information, and . . . expectations depend specifically on the structure of the entire system" (Muth, 1961, p. 315). Muth's contribution potentially had far-reaching implications to the debate initiated by Friedman. Existing theoretical work supporting the efficacy of activist stabilization policy did not assume Rational Expectations, and it was an open question whether there would be substantial changes in the implications of the standard macroeconomic models if Rational Expectations were added to these models.

With the publications of Lucas (1972, 1973), Sargent (1973), Sorgent and Wallace (1975), and Barro (1976), it became apparent that the pro-stabilization policy implications of the standard models were sensitive to the introduction of the Rational Expectations assumption. All these works took variants of Friedman's informal argument and developed formal frameworks within which it could be systematically analyzed. In addition to

the hypothesis that expectations are formed rationally, the models incorporated two other crucial assumptions. First, it was assumed that deviations in production about its natural rate were the result of errors in expectations-formation committed by individuals. Second, it was assumed that all markets cleared within the period of analysis. Given these three assumptions, any countercyclical monetary policy--not just the special case analyzed by Friedman but any systematic policy--was shown to be ineffective.[1] Since Sargent (1973) and Sargent and Wallace (1975) had demonstrated that policy neutrality did not hold when the Rational Expectations assumption was replaced with more traditional assumptions of expectations-formation but the other two assumptions were retained, the possibility could be ruled out that the policy-neutrality results were due solely to the imposition of one of the other two assumptions on the model's structure. Accordingly, the strong policy-neutrality implications of Muth's Rational Expectations hypothesis, when combined with Friedman's emphasis on inflationary expectations, were established.

While Lucas, Sargent, Wallace, and Barro had demonstrated conditions under which systematic monetary policy would be neutral, a source of immediate controversy concerned the extent

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[1]More precisely, "the probability distribution of output will be independent of parameters describing the systematic portion of the authorities' responses to cyclical conditions" (McCallum, 1977, p. 627). Following Waldo, a systematic policy is defined as being "any policy which is known with certainty one period in advance by agents forecasting rationally" (Waldo, 1981, p. 339).

to which these conditions were met in reality, and therefore the extent to which the Rational Expectations models generated results which could be expected to hold in the actual economy. In seeking to evaluate this issue by comparing the apparent implications of the early Rational Expectations models with the historical record contained in the macroeconomic time-series data, an early criticism of these models was based on the widespread presence of serial correlation in these data, so that knowledge of past values of the series would allow a better prediction of current and future values than would be possible in the absence of this knowledge.[2] A notable example of such serial correlation was to be found in the readily-observed phenomenon of the business cycle. Such characteristics in the data seemed to contradict the logic of the Rational Expectations hypothesis, since the early Rational Expectations macroeconomic models apparently implied that there should be no business cycles, a prediction clearly at variance with the evidence.

The case of economic recession can be used to illustrate the general point. Supposing the economy is in recession this period, the unemployment rate in general will be substantially above the natural rate of unemployment. If, as the Rational Expectations models assume, deviations in unemployment from the natural rate are due solely to errors of expectations-formation by individuals, and if, as also seems implied by the Rational Expectations approach, a relatively rapid rate of information

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[2]Two examples of such arguments are Modigliani (1977, p. 6), and Tobin (1980, pp. 36-37).

dissemination concerning past values of key macroeconomic variables exists, then individuals should be capable of observing that unemployment is above the natural rate and should use this information to revise their expectations accordingly. Therefore, by next period individual forecasters should have incorporated their knowledge of this period's high unemployment fully into their forecasts of key variables (such as their real wage), implying that next period's unemployment rate should be independent of this period's rate. Deviations over time by unemployment from its natural rate should then be random events instead of being serially correlated. Arguments of this nature were used to charge that the Rational Expectations approach to monetary theory was logically inconsistent with the existence of business cycles and therefore clearly untenable.

In responding to this criticism, advocates of the Rational Expectations approach advanced counterarguments based on the distinction between "sources of impulses and propagation mechanisms" (Barro, 1981a, p. 48). While Rational Expectations did in fact imply that errors in expectations-formation this period could not directly cause next period's real variables to deviate from their natural rates, such errors this period could affect values of some real variables this period which then, in turn, could cause real variables to exhibit substantial serial correlation even in a Rational Expectations world.[3] The existence of business cycles or of any serially-correlated

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[3]Lucas (1975) presents an early example of a Rational Expectations model built around such reasoning.

macroeconomic time series thus did not necessarily contradict the Rational Expectations hypothesis. However, it did raise the question of whether a plausible case could be made for the existence of some propagation mechanism which could transmit the effects of errors in expectations-formation this period into further future effects in a manner consistent with the Rational Expectations view. Only to the extent that a theoretical case could be built, and persuasive empirical evidence presented, favoring the existence of such a propagation mechanism, could the Rational Expectations approach to monetary theory plausibly be viewed as being consistent with the historical record.

Thus, two important empirical questions were spawned by the initial phase of the Rational Expectations revolution. First, to what degree was the historical record consistent with the policy-neutrality hypothesis? Could the policy-neutrality proposition be defended as being reasonably consistent with the facts? Second (a special case of the first), to what extent could empirical evidence be found which buttressed the case for the existence of some propagation mechanism (or mechanisms) that would resolve the problems alluded to above? That is, how strong was the evidence that the existence of business cycles was not a contradiction of the Rational Expectations/policy-neutrality view? The state of opinion on these two questions will be reviewed in subsequent sections, beginning with the policy-neutrality issue.

Barro's Approach to Testing the Policy-Neutrality Proposition

Assuming (on the grounds outlined above) that it is inappropriate to maintain that the existence of the business cycle alone refutes the Rational Expectations view, a natural attempt to resolve the policy-neutrality debate is to seek to develop a direct test of the explanatory power of the policy-neutrality hypothesis, using formal statistical procedures. While early studies along this line were carried out by Lucas (1973), McCallum (1976), and Sargent (1973, 1976a),[4] the most influential empirical work on the policy-neutrality question has been that of Barro (1977b, 1978, 1981b) and Barro and Rush (1980), which attempts an evaluation of the effects of actual versus unanticipated money growth. This section reviews this empirical work as well as subsequent research following in the Barro-Rush tradition.

Barro's Studies

The essentials of the Barro-Rush empirical procedure are well known. Conceptually, following collection of the appropriate time-series data, the procedure involves two separate stages. First, a money-growth forecasting equation is generated and used to obtain measures of anticipated and unanticipated money growth. Second, the relative abilities of unanticipated money growth on the one hand, and either actual or anticipated money growth on the other, to explain movements over time in real

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[4] A review of this early empirical work on the policy-neutrality hypothesis is outside the scope of this project. A summary discussion of Lucas' (1973) and Sargent's (1976a) work is Barro (1981a, pp. 68-71).

output, unemployment, or other variables of interest, are assessed, using formal statistical tests.

Estimation of measures of anticipated and unanticipated money growth is carried out as follows. Forecasters of future money growth are visualized as knowing how several variables are utilized by the monetary authority in setting money-growth rates. Knowledge of this reaction function and of the values of the variables in that function allows individual forecasters to make rational forecasts of current and future monetary policy. In developing an empirical measure of such a forecasting procedure, the chief problems are, first, deciding which variables belong in the reaction function, and, second, determining the values of the coefficients which are to capture the relationship between these variables and the policy decisions of the monetary authority. Barro solved the first problem mainly by appeal to economic theory, and he solved the second by regressing these several variables against actual money growth over the sample period, each resulting coefficient being taken as an estimate of the relation between changes in a variable helpful in forecasting money growth and changes in monetary policy. Predicted money-growth values from this regression are measures of expected money growth, and the residuals are measures of unanticipated money growth (or money "shocks").

These residuals then are used (along with other variables designed to capture the natural rate of the dependent variable) as regressors in an attempt to explain variations in real output, unemployment, etc., and this regression is interpreted as

indicating the extent to which unanticipated money growth accounts for such variations. Estimation of an analogous equation, with either actual or anticipated rather than unanticipated money growth as the key explanatory variable, and subsequent comparison with the unanticipated money-growth equation, allows an informal assessment of the relative abilities of these two concepts of money growth to explain variations in real output or unemployment. Finally, the policy-neutrality proposition is formally tested. A regression similor to the above, but including both unanticipated and actual (or, alternatively, both unanticipated and anticipated) money growth as explanatory variables, is estimated, and its explanatory power is compared, using F-tests or Likelihood Ratio-tests, with the version using unonticipated but not actual (or, alternatively, using unanticipated but not anticipated) money as the key explanatory variable. A result indicating that the former equation does not explain variations in real output or unemployment "significantly" better than the latter equation, is taken as evidence fovoring the policy-neutrality proposition.

In implementing these procedures, the first problem is the choice of the explanatory variables in the money-growth equation. Barro's results stem from an annual money-growth forecasting equation of the general form

$$DM = a_0 + a_1 DM1 + a_2 DM2 + a_3 FEDV + a_4 LUR1.$$

Here DM, the annual rate of growth of the money supply in period t, is defined as L(M)-L(M1) (L(·) denoting the natural

logarithm), where M is defined as the annual average level of the "M1" concept of the money stock in period t, and M1 (as opposed to "M1") is the value of "M1" in period t-1 (throughout this study, the general notation Xi will be used to denote the value of a variable X in period t-i, and X will denote the value of X in period t). FEDV is a measure of contemporaneous real Federal government spending relative to normal.[5] LUR equals L[U/(1-U)], where U is the unemployment rate in the total labor force (including both civilian workers and military personnel). The measure of abnormal Federal expenditures is designed to capture the incentive for the Federal government to increase the rate of money growth as a method of acquiring revenue; presumably, the more "abnormally" large are real federal expenditures, the greater is the temptation to utilize the "inflation tax" which the government presumes will accompany a pickup in money growth. The lagged measure of unemployment primarily captures the incentive of the government to attempt to stimulate the economy via countercyclical monetary policy.[6] Finally, inclusion of the two lagged money-growth measures is rationalized on grounds that such terms capture any persistence or lagged adjustment not captured by other variables.

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[5] Appendix A, below, gives a more precise definition of FEDV.

[6] Rush (1986, p. 263) points out that, while inclusion of this variable might be interpreted as being inconsistent with Rational Expectations as applied to government behavior, recent work by Barro and Gordon (1983a, 1983b) can rationalize the inclusion of such a variable even in a Rational Expectations/policy-neutrality world.

Letting DME denote the predicted values from the forecasting equation, and defining the residuals from that equation as DMR=DM-DME, these residuals are used as explanatory variables in real output and unemployment equations, along with certain additional variables designed to capture the natural rate of the dependent variable. Barro has achieved success in accounting for variations in unemployment and real output, using specifications of the general form

$$L(Y) = B \cdot Nat_y + b_0 DMR + b_1 DMR1 + \ldots + b_k DMRk,$$

and,

$$LUR = C \cdot Nat_u + c_0 DMR + c_1 DMR1 + \ldots + c_k DMRk,$$

where Y is real GNP, Nat_y and Nat_u are vectors of natural-rate variables (which include the "constant" term for convenience of presentation), B and C are the corresponding vectors of coefficients on these variables, and other variables are as previously defined. Barro's most recent published specifications (Barro, 1981b), estimated over a 1946-78 sample, have k equal to one in both equations and utilize a natural-rate vector composed, for output, of a constant term, a time trend and the {log of the} real value of Federal government expenditures, and composed, for unemployment, of a constant term and the ratio of Federal government expenditures to real output. His earlier specifications (Barro, 1977b, 1978) are slightly different. All, however, exhibit the following characteristics. First, use of Barro's measure of unanticipated money growth generates

specifications for both output and unemployment of good explanatory power. Second, coefficient signs on all variables in his equations are consistent with the predictions of the Rational Expectations approach to monetary theory, with money shocks being inversely related to unemployment and directly related to output, and with coefficients on natural-rate variables also conforming to prior predictions. Third, comparing the above with alternative specifications where the DMRs are replaced by somewhat longer lags of actual money growth (DM), strongly suggests that money shocks have explanatory power superior to that of actual money growth in real output and unemployment equations.

Fourth, Barro's formal test of the policy-neutrality hypothesis leads to results which, overall, support the hypothesis for annual data over the U.S. postwar era. Barro's test involves two main steps: first, comparing the explanatory power of an equation containing both DMs and DMRs with that of an equation containing only the DMRs; and, second, as a means of assessing the power of the previous test, adopting the reverse procedure of comparing the explanatory power of an equation containing both DMs and DMRs with that of an equation containing only the DMs. Results favorable to the policy-neutrality hypothesis are the following: first, a failure to reject the null hypothesis that deletion of the DMs does not significantly reduce performance; and, second, the rejection of the null hypothesis that deletion of the DMRs does not significantly reduce performance. While results are not entirely unambiguous,

on balance they support "the hypothesis that actual monetary growth is unimportant for the determination of real variables, given the values of the monetary shocks DMR" (Barro, 1981b, p. 147).

However, in assessing the extent to which Barro's empirical results can be interpreted as supporting the policy-neutrality hypothesis, an important characteristic of these results is the persistent response of unemployment and output to unanticipated money growth, and, at least in the case of unemployment, a response also exhibiting what Kydland and Prescott (1980, pp. 171-172) have called momentum. In the context of regression analysis, a persistent response to money shocks will here be taken to mean that a lagged money-shock coefficient is statistically significant at the five percent significance level in explaining variations in a dependent variable. Such a persistent response also will be said to exhibit momentum if the (statistically significant) coefficient on DMR(j-1) "substantially" exceeds the coefficient on DMRj (for annual data j usually is set equal to t). Barro refers to this pattern of shock coefficients, which first rises and then falls over time, as exhibiting a "triangular" coefficient pattern (Barro, 1981b, p. 145).

The presence of persistence and momentum in the Barro equations has prompted critics of the policy-neutrality hypothesis to dispute the notion that Barro's results constitute evidence supporting the hypothesis. Their argument (for example, as in Blinder, 1980, p. 53) is a special case of that discussed

in the previous section, which alleges that serial correlation in the macroeconomic time-series data--and in particular the existence of business cycles--constitutes a refutation of the Rational Expectations view. The argument can be applied to the Barro empirical results in the following manner. If, as is typically assumed in the Rational Expectations approach, individuals observe the actual value of money growth with a one period lag, then they ought to be capable of comparing this value with what had been last period's expected rate of money growth. In this way individuals should deduce the unanticipated movements in money that occurred in previous periods, so that knowledge of the existence in previous periods of these money shocks becomes part of agents' current-period information sets. Under these circumstances, why should rational individuals make a real response in the current period to what is now a fully recognized shock from a previous period? Such a real response apparently clashes with the logic of the Rational Expectations hypothesis.

As is the case in the Rational Expectations/business cycles dispute, attempts to reconcile the statistical significance of lagged money shocks in Barro-type equations with the notion of Rational Expectations center around arguments for the existence of some propagation mechanism which is theoretically capable of translating short-term shocks into longer-term fluctuations in real variables. Such counterarguments stem from the observation that the Barro equotions are reduced form, rather than structural, equations, so that Barro's empirical results give relatively little information about the specific economic

structure which gives rise to the persistent impact of money shocks. In particular, a previous-period unanticipated money movement might affect the values of real variables in that previous period, which then, in turn, would affect the current-period values of real variables. On this interpretation, lagged shocks are best interpreted as proxies for unidentified "state" variables, the inclusion of which would, in principle, entirely account for the persistence of such shocks. Thus the impact of lagged money shocks on current real variables is conceived by advocates of this view as being indirect, operating in a way which does not necessarily contradict the logic of the Rational Expectations hypothesis. While it is clear that such an interpretation can account for the persistence of money shocks in principle, it is equally clear that building a case for the existence of such a propagation mechanism is critical to those who would use Barro's empirical results to bolster the case for policy neutrality. In the absence of such a case, Barro's results plausibly can be interpreted as being evidence against, not for, the policy-neutrality hypothesis.

Other Results Favoring Policy Neutrality

Barro's pioneering studies triggered new empirical work using his techniques which explored whether his results also held true when different samples of U.S. data were analyzed and when the analysis was extended to other countries. A number of researchers utilizing Barro's basic approach have found results to be quite robust to these variations. However, generally this evidence also has continued to indicate the presence of a

persistent impact of money shocks, a finding which underlines the importance of the propagation-mechanism question discussed above.

It will be convenient to begin with studies of the United States economy and then move to studies of other nations. Analysis of post-World War II quarterly U.S. data using the Barro testing procedure has been carried out by Barro and Rush (1980).[7] For both output (1947:I-1978:I) and unemployment (1949:I-1978:I), Barro and Rush find a close correspondence between results derived from annual and quarterly data. Results for output using Ordinary Least Squares include "a strong contemporaneous response (to money shocks), a peak effect with a 3-4 quarter lag, a strong persisting effect through two years, and no significant remaining effect after 10 quarters" (Barro and Rush, 1980, p. 34). The equation also performs well in other respects, excepting the presence of substantial serial correlation in the residuals. Adjustment for serial correlation reduces the measurement of the contemporaneous money-shock effect, and shortens the observed persistence to seven quarters. Results for unemployment are similar to those for output. Barro and Rush do not carry out formal testing of the policy-neutrality hypothesis for quarterly data. Still, the acceptable performance of their specifications, plus their finding that unemployment and output each exhibit both persistence and momentum in their responses to money shocks, are consistent with Barro's earlier results obtained over this sample period using annual U.S. data.

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[7] 1941-45 quarterly data is added to the postwar sample in carrying out the estimation of the money-growth prediction equation.

Rush (1986) has analyzed the response of U.S. unemployment to monetary-base shocks using annual data over the 1920-83 period. Monetary-base shocks rather than "M1" shocks are used to control for the possible endogeneity of "M1" over the sample; in other respects, the approach closely follows that of Barro. Rush analyzes two subsamples within the 1920-83 period, one (1920-30, 1946-83) omitting both the Great Depression and World War II years, and the other (1920-40, 1946-83) omitting only the World War II years. Rush gets good results for the first subsample using a right-hand-side specification for the vector of natural-rate variables identical to Barro's specification for output. For the second subsample (which includes the Depression years), Rush finds that the addition of a money-multiplier variable to his right-hand-side specification leads to good performance over this sample as well. Results for the first subsample are characterized by both persistence and momentum in response to monetary-base shocks extending forward two years from the date of the shock. Results for the subsample including the Great Depression years are characterized by persistence extending forward two years from the date of the shock. Finally, Rush tests for whether unanticipated monetary-base growth outperforms anticipated monetary-base growth in explaining variations in unemployment. For the first subsample, results support the policy-neutrality hypothesis. Rush does not report results for the second subsample.

Evans (1984) has used a version of Barro's methodology in his study of the effects of interest-rate and money-growth

volatility on United States real output in the postwar era. Evans uses a vector of natural-rate variables that differs from that used by Barro: He uses a measure of unanticipated interest-rate volatility and (sometimes) a measure of unanticipoted money-growth volatility in addition to variables used by Barro. He also makes use of lagged real output as a right-hand-side variable. In other respects the approach follows that of Barro. Analysis is carried out for two subsamples of annual data: 1947-78 and 1947-81. For both subsamples, Evans finds that real output responds to "M1" shocks with persistence extending one year forward in time; in addition, this finding is robust to a number of changes in specification. Evans does not carry out tests comparing the explanatory power of money shocks with either actual or anticipated money growth.

Rush (1985) adapts the Barro procedures to analysis of the U.S. Gold Standard era (1880-1913 annual data). The findings are interesting in that, if taken at foce value, they provide evidence suggesting that neither actual nor unanticipated money growth plays an important role in explaining variations in real output over this sample. These results are consistent with the policy-neutrality hypothesis (since there is no evidence that either monetary variable hos a substontial impact on real output in this era), but contradict findings for more recent U.S. samples. The fact that results are not robust to this change in sample also provides evidence which strengthens the argument for accepting the other studies' results at face value. This is because there are good theoretical grounds for believing that the

impact of monetary change was less in the Gold Standard era (as discussed by Rush, 1985, p. 320 and the references cited there), and, when viewed in this context, Rush's results suggest that there is nothing in the Barro procedures which biases results towards finding a statistically significant role for DMRs. An alternative interpretation of Rush's negative findings is suggested by Blejer and Fernandez' study of Mexico (below, pp. 31-33).

A number of papers also have been published which adapt the Barro methodology to the study of other countries. Bellante, Morrell, and Zardkoohi (1982) analyze the United Kingdom case, for annual data over the 1946-77 period. Money shocks are generated using Barro's forecasting-equation specification. For unemployment, using a measure of union membership as a natural-rate variable, a current and three lagged values of shocks are found to be statistically significant. Results thus indicate a persistent response of unemployment to money shocks; in addition, substantial momentum characterizes the results for unemployment. Results for output also indicate persistence (extending two years forward) and momentum. The vector of natural-rate variables in the output equation includes a union membership variable and a time trend. Bellante, Morrell, and Zardkoohi also test the explanatory power of unanticipated versus actual money growth, and, for both unemployment and output, find that deletion of actual money growth, given the inclusion of the money shocks, does not significantly reduce the performance of their equations

(Barro's reverse test stemming from deletion of the DMRs while including the DMs does not appear to have been carried out).

Attfield, Demery, and Duck (1981b) carry out an analysis of U.K. real output which differs only slightly from that of Bellante, Morrell, and Zardkoohi. Analysis again is for annual U.K. data over the 1946-77 period; however, a different money-growth prediction equation is utilized, and a different vector of natural-rate variables is employed. Right-hand-side variables in the money-growth prediction equation are the real value of the public-sector borrowing requirement, the one-period-lagged real current-account balance of payments surplus, and two lagged values of money growth. Right-hand-side natural-rate variables in the real output equation are a time trend and a measure of inflation-rate variability. Results indicate a persistent response of real output to money shocks which extends forward three years after the date of the shock. In addition, results indicate the presence of momentum in the pattern of response of real output to money shocks. A Durbin-Watson statistic in the low end of the indeterminate range, however, makes the interpretation of these results difficult. Attfield, Demery, and Duck also report an equation where actual money growth is substituted for money shocks, and maintain that "total monetary growth (i.e., anticipated and unanticipated) does not enter the output equation satisfactorily" (Attfield, Demery, and Duck, 1981b, p. 373). However, no formal test of this proposition is carried out.

Attfield, Demery, and Duck (1981a) also have adapted the approach discussed immediately above to the analysis of 1963-78 quarterly U.K. data. Explanatory variables in their money-growth forecasting equation are the real value of the government's borrowing requirement, the real value of the lagged current account surplus, one- to three-quarter-lagged values of DM, and three quarterly dummy variables. Their equation for real output exhibits persistence extending five quarters forward in time after the date of the shock, as well as substantial momentum (a shock's peak effect occurring five quarters after its occurrence). The equation performs well in other respects: There is, for example, no strong evidence of first- through fourth-order serial correlation in the residuals of the output equation. Although estimated for a different sample, it is still worth noting that Attfield, Demery, and Duck's quarterly results are consistent with their annual results for the U.K. (discussed above).

Wogin (1980) has adapted Barro's basic approach to a study of Canadian unemployment. Annual data over the 1927-72 period is utilized in generating the money-growth prediction equation, and 1927-39, plus 1948-72, annual data is used in estimating the unemployment equation. As right-hand-side variables in his money-growth equation, Wogin uses one-period-lagged measures of the five variables Federal spending, GNP, exports, unemployment, and money growth, as well as a dummy variable (for the 1940-47 war years). A right-hand-side specification for the unemployment equation consisting only of the lagged unemployment rate and

money shocks generated a well-behaved equation which exhibited a persistent response extending one year forward in time. An alternative right-hand-side specification which added contemporaneous Federal spending and contemporaneous exports as explanatory variables generated an equation which did not exhibit persistence in response to money shocks (however, in this latter case, no statistically significant role for anticipated money growth could be found either). Alternative specifications substituting anticipated for unanticipated money growth appear to perform less satisfactorily, but no formal test of the relative explanatory power of the competing concepts of money growth is carried out. Wagin's results provide some evidence that the Barro analysis yields similar results for Canada, but his results are weaker than those found by Barro for unemployment in the U.S.

Some work also has been published which attempts to adapt the Barro procedures to the study of several "third-world" economies. Barro himself (Barro, 1979) has analyzed the cases of Mexico, Colombia, and Brozil over a portion of the postwar era. Analysis was most satisfoctory for the case of Mexico. Barro generates a money-growth prediction equation of acceptable performance, for annual observations over the 1948-73 period. Right-hand-side variables are the following: three lags of Mexican money growth, contemporaneous U.S. money growth, and a lagged measure of the departure of Mexican prices from purchasing power parity. Analysis of the behavior of output is carried out over a 1954-73 annual sample. Natural-rate variables are the following: the lagged value of U.S. output, the absolute value

of the departure of the Mexican/U.S. exchange rate from purchasing power parity, a measure of Mexican terms of trade, and a time trend. Analysis of output using this natural-rate specification plus a current and two lagged money-shock measures leads to an equation of acceptable performance. Output exhibits both persistence and momentum in response to money shocks. However, Barro fails to find evidence favoring the proposition that unanticipated money growth outperforms actual money growth in explaining real output. Barro's results for Mexico, in sum, indicate that money shocks are acceptable explanatory variables in the Mexican case, and that money shocks generate both persistence and momentum, but fail to support policy neutrality. Results for Colombia and Brazil are much less satisfactory, as Barro fails to find evidence suggesting that his approach yields useful results of any kind for these two countries.

Barro's results for Latin American open economies thus do not support the policy-neutrality hypothesis. However, these results were obtained using the methodology he had developed for the closed U.S. economy. Blejer and Fernandez (1980) have revised Barro's approach to make it more suitable to the study of an open economy, and they find results for Mexico which are much more favorable to the policy-neutrality hypothesis than are those of Barro. Blejer and Fernandez introduce two key modifications to Barro's approach. First, since in a fixed-exchange-rate open economy the nominal money supply is an endogenous variable, the appropriate monetary instrument is not the money supply but rather that component of the money supply which can reasonably be

taken as exogenous; specifically (for the case of Mexico), the domestic-credit component of the monetary base. Second, Blejer and Fernandez hypothesize that unanticipated growth in this monetary aggregate will have a direct effect only on the output of the nontraded-goods sector. Since producers of goods in the traded-goods sector are price takers, a positive money shock will increase the prices of nontraded goods but not of traded goods, raising nominal wages throughout the economy and so depressing activity in the traded-goods sector, where a compensating rise in prices does not occur. Therefore, in the open economy, one should look for the stimulative effects of positive money shocks primarily in the nontraded-goods sector (and the depressing effects of negative money shocks primarily there as well). Also, since in the open economy a money shock has opposite effects on the traded- and nontraded-goods sectors, one might not expect to observe a response of real output (traded plus nontraded goods) to money shocks. This latter result might explain the failure of Rush (1985) to observe a effect of money shocks on real output during the U.S. Gold Standard era.

Having made these adjustments, Blejer and Fernandez study the Mexican economy using 1950-75 annual data. The authors depart substantially from the Barro methodology in generating their estimate of money shocks, as they use time-series methods in generating their series. In their second-stage estimates, they employ a vector of natural-rate variables made up of, first, a time trend, and, second, the difference between (the log of) traded-goods prices and (the log of) nontraded-goods prices.

Adding a contemporaneous money-shock term yields their right-hand-side specification (thus, Blejer and Fernandez's study gives no information on the persistence/momentum issue, since lagged shocks are not used as explanatory variables in their work). Results are as predicted by their theory: first, little impact of contemporaneous money shocks on traded-goods output; and, second, a substantial impact of contemporaneous money shocks on nontraded-goods output. In addition, the equation for the nontraded-goods sector performs much better than that for the traded-goods sector (as measured by comparing standard errors of the two regressions and Durbin-Watson statistics). Substitution of anticipated money growth for unanticipated money growth in the nontraded-goods equation yields a coefficient on expected money growth which fails to achieve statistical significance and which possesses the wrong sign; further, this change generates a substantial deterioration in the overall performance of their equation. Blejer and Fernandez thus provide some evidence that the policy-neutrality proposition is relevant to third-world economies.

Additional evidence relevant to the policy-neutrality debate, stemming from the analysis of a number of countries, is provided by Attfield and Duck (1983). The authors use 1951-78 annual data for the United States, the Netherlands, Canada, Denmark, Australia, the United Kingdom, the Philippines, Colombia, El Salvador, Guatemala, and Argentina. A money-growth prediction equation and an equation for real output are generated for each country. Explanatory variables in the money-growth

prediction equations are real government consumption and (for most countries) also a one-period-lagged value for money growth (in the case of the U.K., a second lagged money-growth term also is included). The output equations utilize a time trend and the lagged value of output as explanatory variables. Both a contemporaneous and a one-period-lagged money-shock term is tried as an explanatory variable in the second-stage equation for each country. For all countries the contemporaneous shock term is statistically insignificant, although it is close to being significant in the cases of the Philippines and Colombia (for these two countries the reported specification is with a contemporaneous shock term only). For the remaining nine countries the lagged shock term substantially outperforms the contemporaneous term and (the authors explain) therefore the specification reported contains only a lagged shock term. A statistically significant lagged money-shock coefficient is reported by the authors for the U.S., the Netherlands, Canada, Denmark, and the U.K., meaning that both persistence and momentum is observed in these countries. Australia and Guatemala fail to generate statistically significant coefficients on the lagged shock term but these coefficients are fairly close to being significant (at the five percent significance level). Only El Salvador and Argentina clearly fail to generate suggestive results. Attfield and Duck carry out neither informal nor formal tests of the relative explanatory power of money shocks versus either actual or expected money growth. Still, the authors'

results represent further evidence of the substantial robustness of Barro's basic approach.

In summary, there is a considerable body of empirical evidence suggesting that current and lagged measures of unanticipated money growth can successfully explain variations in real output and unemployment. Further, a number of these studies have presented evidence of varying strength suggesting that unanticipated money growth outperforms either actual or anticipated money growth in explaining variations in real output and unemployment. These results are robust across a number of U.S. data samples and across various samples taken from a number of other nations.

However, in the bulk of this research there is also strong and robust evidence of a persistent response of real output and unemployment to money shocks. Such persistence can be argued to be inconsistent with the Rational Expectations/policy-neutrality view on the grounds presented above (pp. 21-22). Therefore, to interpret these studies as representing evidence which supports that view is to presume that the persistence of money shocks can be accounted for by reference to some "propagation mechanism," which converts short-term money shocks into longer-term real disturbances. The empirical findings reviewed above can be taken as evidence favoring the policy-neutrality hypothesis only if a persuasive case exists which supports the case for the existence of such a mechanism (or mechanisms).[8]

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[8]The relevance to the present study of the literature criticizing Barro's policy-neutrality test is discussed in note 22, below.

Reconciling Policy Neutrality with Persistence: Three Influential Propagation Mechanisms

The previous two sections have established that the theoretical and empirical case for the Rational Expectations/policy-neutrality view rests in part on a resolution of the persistence question. Specifically, why should lagged nominal shocks have a persisting impact on real variables given that expectations are formed rationally and information lags are short? A number of theoretical conjectures have been advanced which are capable, in principle, of accounting for persistence in a manner consistent with the policy-neutrality hypothesis. Arguably the two most influential have been, first, the "time-to-build" mechanism of Kydland and Prescott (1982), and, second, the inventories-based mechanism of Blinder and Fischer (1981). A third mechanism--the "wage-stickiness" mechanism of Fischer (1977a)--also can account for persistence, but in a manner that is not unambiguously consistent with the policy-neutrality hypothesis (below, pp. 67-69). This section reviews the theoretical justification and empirical evidence supporting each of these propagation mechanisms. It will be seen that, while each argument is theoretically sound in the sense that each is capable in principle of accounting for the persistence phenomenon, empirical evidence regarding the consistency with the data of the mechanisms is fairly scanty.

The "Time-To-Build" Propagation Mechanism

The idea that "the assumption of multiple-period construction is crucial for explaining aggregate fluctuations"

(Kydland and Prescott, 1982, p. 1345) is neither new nor uniquely associated with the Rational Expectations approach to macroeconomics: As early as 1937, Hayek analyzed the implications of the assumption to business cycle theory.[9] However, Kydland and Prescott (1982) were the first to cast the idea explicitly into a Rational Expectations "equilibrium" framework. Kydland and Prescott construct a Rational Expectations model where "only a small fraction of additions to the capital stock that are decided on in a given year show up as investment expenditures in the same year" (Kydland and Prescott, 1980, p. 177), and where "more than one time period is required for the construction of new productive capital" (Kydland and Prescott, 1982, p. 1345) (these two assumptions define "time to build" as that phrase will be used in the present study). Kydland and Prescott argue formally that the assumption does in fact generate a persistent response by real variables to technology (supply) shocks. While Kydland and Prescott do not investigate the potential for demand shocks to generate persistence in such a model, conceptually it is but a short step to such a framework (although, due to technical constraints, such a model has not yet been constructed).

The time-to-build propagation mechanism has been received favorably by leading advocates of the Rational Expectations

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[9]The key idea is stated by Hayek as follows: "The first investment of such a chain, therefore, will be undertaken only if it is expected that in each link of this chain a certain rate of interest can be earned. But this does not mean that, once this investment has been made, the process of further investments will not be continued if conditions change in an unfavorable direction" (Hayek, 1975, p. 75).

"equilibrium" approach to the analysis of the business cycle. Sargent (1987, p. 49-51) has analyzed the characteristics of a model similar to that of Kydland and Prescott, while Lucas (1987) has given the Kydland-Prescott model extended attention. Lucas describes the model as

> a highly simplified, competitive system, in which a single good is produced by labor and capital with a constant returns technology. All consumers are assumed to be infinitely-lived and identical. The only 'shocks' to the system are exogenous, stochastic shifts in the production technology. Kydland and Prescott ask the question: 'Can specific parametric descriptions of technology and preferences be found such that the movements induced in output, consumption, employment and other series in such a model by these exogenous shocks resemble the time series behavior of the observed counterparts to these series in the postwar, U.S. economy?' This seems to me exactly the right question for macroeconomists to ask. (Lucas, 1987, pp. 34-35)

Three crucial assumptions are added to this basic framework. First, preferences of consumers are assumed to depend not just on current-period leisure but on a distributed lag of current and past leisure, which increases the intertemporal-substitution possibilities of consumers. Second, time-to-build capital-production conditions are assumed to hold. Third, the technology shocks are assumed to consist partly of permanent and partly of transitory components, mixed together in a way producers cannot observe with certainty. The setup and solution of the resulting model involves dynamic programming and is outside the scope of present discussion. However, Lucas summarizes the characteristics of the solution as follows:

> The artificial time series so generated by the theoretical model 'look like' economic time series . . . the variables show erratic, serially correlated fluctuations about their mean values. . . . (A)

> favorable technology shock shifts out current production possibilities; this induces high capital accumulation which spreads this benefit forward into future periods. . . . It is instructive to run a simulated 'boom' through the Kydland and Prescott model. Suppose a high technology shock occurs, increasing the current productivity of both capital and labor. This makes the current period attractive to work and produce, relative to conditions that are expected to prevail in future periods, so both employment and output rise. It also <u>may</u> signal high productivity in future periods, and the only way for firms to hedge against this (attractive) contingency is to initiate investment projects now. The projects so initiated will operate to increase output and employment until they are completed, spreading the effects of this shock--even if it should turn out after the fact to be transient--forward into future periods. (Lucas, 1987, pp. 39-42)

Contained within the mechanism are two potential sources of persistence: an investment (demand-side) effect, and a capital-stock (supply-side) effect. First, during the "gestation period" for capital more investment expenditures will be engaged in than would have occurred in the absence of the (positive) shock, which tends to increase overall production during these intermediate periods. Second, at the end of the "gestation period" a greater quantity of productive capital will be in existence than would have existed in the absence of the (positive) shock, implying the possibility of greater overall production in these future periods due to lower marginal costs of production.[10]

As previously mentioned, the Kydland-Prescott model is one where real, technology shocks rather than nominal, monetary shocks drive the mechanism, so that the model does not directly

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[10]Lucas (1975) develops an equilibrium business cycle model based in part on such an effect.

relate to the question of why real variables respond to money shocks with persistence. However, Lucas' view is that

> it does not seem to me at all implausible that in a model such as Kydland and Prescott's, elaborated so as to admit limited information on the part of agents, that shocks of monetary origin would be 'misread' by agents as signaling changes in technology or preferences, and hence trigger the same kind of dynamic response that technology shocks do in the model they reported. Indeed, it is hard to imagine how it could be otherwise. (Lucas, 1987, p. 100)

One mechanism by which the above could occur has been developed (informally) by Barro (1984, pp. 460-471). A positive "global" money shock generates price increases across a number of "local" products or markets, which are interpreted by imperfectly informed suppliers as representing preference changes by demanders in favor of these suppliers' products. Given that suppliers believe that such a change in preferences has occurred in the present period, it is reasonable for them to presume that the new pattern will persist for several successive periods, since most preference changes are not "temporary."[11] This argument implies that suppliers will have an incentive to alter their patterns of investment expenditures so as to increase their stock of productive capital in future periods.

The Kydland-Prescott mechanism--altered as suggested by Barro and Lucas to incorporate the effects of nominal-demand shocks--thus works in the following manner. A positive shock in

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[11] Specifically: "The increase in consumer demand tends also to raise the prospective relative price, $P_{t+1}(z)/P_{t+1}$. The main reason is that the high demand typically persists for awhile. (Think again about new products, such as video cassette recorders or six-foot TV screens.) Further, we assume that the entry of new suppliers is insufficient to return the relative price to unity within a single period" (Barro, 1984, p. 467).

period t causes suppliers to expect superior selling opportunities in future periods, a development which raises their optimal capital stock for these periods. They thus initiate multiperiod capital-construction projects in t. Even though, in period t+1, these suppliers recognize the true nature of the nominal-demand shock that occurred in t, the projects initiated in period t are not halted (although given full information in t they would not have been begun). Therefore the positive shock in t generates increased investment demand for the length of the capital "gestation period." In addition, when the capital is complete, there is the possibility of relatively cheap production possibilities leading to additional production of goods in general. The reverse effects take place in the event of a negative nominal-demand shock: Fewer projects are started in t, meaning there will be less investment during the intermediate "gestation periods," and possibly there will be more expensive production eventually due to less capital having been started in t. In both cases, short-term nominal-demand shocks generate serially correlated movements in real output for several periods after the date of the shock. These movements take place despite the existence of only a single-period information lag, so that persistence is explained in a manner consistent with the Rational Expectations hypothesis.

Two key implications of the time-to-build mechanism can be observed in the above description. First, the mechanism implies that the lag separating the decision to invest from the completion of capital is responsible for the persistence of

nominal-demand shocks. Accepting this conclusion suggests that at least some of the missing "state" variables in the Barro empirical research (reviewed in the previous section) are variables which capture the "gestation periods" associated with the construction of capital goods. Therefore, the proper inclusion of such variables in Barro-type regression equations should reduce substantially the persistent impact of money shocks if the time-to-build mechanism is a main explanation of such persistence.

Second, the time-to-build mechanism presumes that the "gestation periods" referred to above are associated with the actual construction of capital, rather than with decisions by suppliers to start the construction of capital. The reasons underlying such a presumption have not been spelled out in the literature on the time-to-build mechanism, but they are straightforward. The idea of a "gestation period" associated with "starts" which lasts for a number of periods is much less persuasive than it is when such an idea is applied to the actual construction of capital projects. First, there is no empirical evidence suggesting that the technological problems associated with the start of a new project are particularly time-intensive. Further, on the presumption that "starts"-related expenditures are a small proportion of the total expenditures leading to the eventual construction of the completed capital, under Rational Expectations an intention to start--brought about due to a positive shock in t--should not be carried out in t+1, when full information about the events in t is assumed to be available. On

the above interpretation, the persistence of "starts" would thus represent evidence that something other than--or at least in addition to--the Kydland-Prescott time-to-build mechanism is chiefly responsible for the persistent impact of money shocks.[12]

Empirical evidence on the length of the capital-construction process

The key necessary condition of the time-to-build propagation mechanism is that the capital-construction process be relatively time-intensive in nature. There is a substantial body of evidence supporting this presumption.

Econometric studies of investment behavior. There are a large number of empirical studies which test the explanatory power of various structural models of the investment process. Examples are Jorgenson and Stephenson (1967a, 1967b), Bischoff (1970) and, more recently, Clark (1979). Surveys of much of this literature have been published by Jorgenson (1971) and Hall (1977). Typically these studies postulate an investment equation of the general form

$$I = b_1(KD - KD1) + b_2(KD - KD2) + \ . \ . \ . \ + b_s(KD - KDs) + D,$$

where I is some concept of gross investment in period t, KDi is the desired capital stock as of period t-i, and D is depreciation in period t. Jorgenson (1971, p. 1112) notes that the theory of investment summarized in the above equation consists of three

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[12]However, some short-term persistence of "starts" might be reconciled with the Kydland-Prescott version of the time-to-build mechanism. This issue is discussed in greater detail in Chapter 3 (pp. 128-129, below).

components: a theory of the desired capital stock, a theory of replacement investment, and a theory of the relationship between changes in the demand for capital services and actual investment expenditures. Net investment is modeled as a weighted average of past changes in the desired capital stock, and replacement investment typically is assumed to be proportional to the actual capital stock K. A wide variety of specifications of the desired capital stock have been adopted and are reviewed in detail by Jorgenson (1971).

The time-to-build propagation mechanism can be expressed in the context of the above setup as a two-part hypothesis. First, the mechanism implies that the b_i--which determine the relationship between investment and changes in the desired capital stock--can be fully accounted for in terms of the period-by-period construction progress patterns for capital goods. Second, the mechanism implies that the desired capital stock in period t+j is a function of whatever nominal-demand shock has occurred in t+j, but that KD is not a function of nominal-demand shocks occurring in previous periods. It is primarily with respect to the first of these implications that the empirical investment literature is of interest in the present context. Results of these studies regarding the lag between a change in the determinants of the desired capital stock and actual investment expenditures are summarized by Hall, who states that

> except for a number of studies with obvious econometric problems associated with the use of Koyck distributed lags without correction for serial correlation, there is remarkably close agreement about the basic features of the lag functions. They are smooth, hump-shaped distributions with an average lag of about two years.

> . . . Within the general class of flexible accelerator investment models, this conclusion seems to hold over quite wide variations in the specification of the demand function for capital and in the econometric method used to estimate the lag distributions. (Hall, 1977, p. 84)

The close correspondence between these lags and those found by Barro (1977b, 1978, 1981b) and Barro and Rush (1980) in their reduced-form equations for real GNP and unemployment is suggestive. Further, the considerable success in fitting distributed lags to investment expenditures represents fairly strong evidence of the time-consuming nature of the investment process. However, when moving from the findings in the empirical investment literature to the question of the evidence favoring the time-to-build propagation mechanism, several caveats are in order. First, as Hall indicates in a passage immediately following the above citation, the assumptions underlying the body of results summarized above have not gone unquestioned.[13] Second, the empirical investment literature does not directly yield information on the proportion of the distributed lag which is due to time to build and the proportion due to other factors, such as lags between the change in the desired capital stock and the time of project start, or even such as possible lags in the response of the desired capital stock to its determinants.

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[13]Hall takes the view that "of course, all of this evidence is subject to the potentially serious bias from endogeneity (of real output) discussed earlier. Though some studies have used simultaneous estimation techniques, none to my knowledge has come to grips with the basic obstacle that the logic of the distributed-lag investment function makes any lagged endogenous variable ineligible as an instrument unless it is lagged more than the most distant part of the investment lag distribution." (Hall, 1977, p. 84)

A third caveat is of particular importance. The empirical studies of investment referred to above attempt to measure the time structure of the response of investment to a change in its long-term determinants. Thus the presumption underlying these studies is that there has been a change in the desired capital stock due to a permanent change in some "real" factor. The analysis thus is not necessarily relevant to the issue of the response of investment to a short-term nominal shock which is presumed to be recognized as such in the next period. For example, a short-term nominal shock in period t might cause plans to start a project to be begun in t, but it is reasonable to expect such plans to be cancelled in t+1 given only a single-period information lag. Still, on the presumption (supported by survey data reviewed below) that a substantial proportion of the lag separating investment from a change in the desired capital stock is due to technological, production-related factors, the studies cited above do have relevance to the question of the evidence favoring the time-to-build propagation mechanism.

Survey data on the time structure of the investment process. Additional evidence of the time-consuming nature of the investment process is to be found in several independent sources of survey data. Mayer (1960), in a 1954 survey of over a hundred U.S. companies then building industrial plants, electric power plants, or plant additions, found that an average of five quarters elapsed between the start of construction and the completion of the facility. Since the time of Mayer's pathbreaking study, the U.S. Department of Commerce has collected

an extensive body of data, from the early 1960s to the present, for nonresidential and residential construction regarding average production periods, monthly construction progress patterns, and the number of months from start to completion for these investment concepts (the first publication of these studies is in U.S. Department of Commerce, 1970). These data are reviewed in detail in Appendix B and in the discussion surrounding Tables 3-5 and 3-10 (below, pp. 111 and 125). They indicate that, on average over the 1961-83 period, 63 percent of the value of nonresidential construction projects was in place within four quarters of the start of construction, and 93 percent was in place within eight quarters of project start. For residential projects, on average over the 1964-85 period, 77 percent of the value of such projects was completed within one quarter of project start, and 96 percent within four quarters of project start. The Commerce department findings are very consistent with those of Mayer, once allowance is made for the smaller average project size in the Commerce department sample. In sum, the available survey data confirm the premise that lengthy construction periods make up a substantial portion of the total time required to procure completed capital.[14]

While a substantial body of survey data are available giving information on the length of capital-construction periods, relatively little such data are available regarding the length of

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[14]Survey data on production periods for producers' durable expenditures are both less substantial and less supportive of the hypothesis of lengthy production periods for this category. These data are discussed in detail in Chapter 3 (pp. 112-122, below).

the lag separating the date of a (presumed) change in the desired capital stock from the date of the start of construction. Mayer's (1960) survey indicates that, on average for his sample, slightly more than two quarters (7 months) elapsed between the date at which the drawing of plans for a project began and the date of project start. The seven month interval is broken down as follows. One month after the drawing of plans began, the final decision to build was made. Four months after the start of the drawing of plans marked the date of completion of external financing to cover the cost of construction. Finally, five months after the start of the drawing of plans came the placing of the first significant orders. Mayer's survey does not provide evidence regarding the important question of the length of the lag preceding the start of the drawing of plans. However, if this last lag can be presumed to be short (say, one month),[15] then Mayer's findings indicate that, at least for his sample, the final decision to build came within a quarter of the change in the determinants of the optimal capital stock. Such a short lag would put the final decision to build within the confines necessary in order for a short-term nominal-demand shock to induce those starts of projects which is a necessary condition of the time-to-build propagation mechanism. However, under these

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[15] An argument for such a presumption stems from Lucas (1977): "For individual investment projects, rates of return are highly variable, often negative, and often measured in hundreds of percent. A quick, current response to what seems to others a weak 'signal' is often the key to a successful investment. The agent who waits until the situation is clear to everyone is too late; someone else has already added the capacity to meet the high demand" (Lucas, 1977, p. 23).

circumstances an explanation still is lacking as to why the "final decision" to build should in fact be final; that is, why would cancellation of such a project prior to start not be optimal given that it is known that the plans to start the project were mistakenly induced by a nominal-demand shock? Mayer's results would thus appear to fall into an ambiguous range with respect to the question of whether business decisions to start investment projects display too much persistence to be consistent with the time-to-build mechanism.

Empirical testing of the time-to-build propagation mechanism

As is indicated in the above discussion, the time-to-build mechanism has been well-received by the advocates of the Rational Expectations "equilibrium" approach to business cycle research. Kydland and Prescott (1982) have rigorously established the theoretical basis for the mechanism as a potential explanation of business cycles in a model where technology shocks drive the system, and Lucas (1987) and Barro (1984) have speculated in intuitively appealing ways as to how nominal-demand shocks might be fit into such a model. Further, there is a good bit of empirical evidence indicating that various types of capital do take substantial amounts of "time to build," evidence which adds considerably to the appeal of the time-to-build mechanism. However, there is very little empirical work which actually tests the explanatory power of the time-to-build mechanism against the historical record, and there has been no published attempt to assess its explanatory power in comparison with that of other plausible propagation mechanisms. In the absence of such

testing, the time-to-build mechanism can at best be viewed as a theoretically appealing conjecture loosely supported by some well-known facts about the behavior of investment expenditures over the business cycle.

The only "empirical test" of the time-to-build propagation mechanism published to date is that of Kydland and Prescott (1982). The work is best described as a simulation experiment that uses some independent data sources to fix the values of key parameters. Lucas describes the Kydland and Prescott testing procedure as follows:

> Kydland and Prescott began by estimating as many parameters as possible from a wide variety of out-of-sample evidence. For example, the fact that people work about one-third of the time pinned down one preference parameter; the observation that investment projects take something like a year to complete was used to fix a technological parameter; and so forth. Having estimated as many parameters as they could in this way, without even looking at the time series they were attempting to fit, the number of free parameters--including the critical parameters characterizing the technology shocks that drive the system--was reduced to about six. Kydland and Prescott then chose values for these remaining parameters so as to make certain low order moments (variances, covariances, autovariances) predicted by the model 'match' the corresponding moments from the collection of time series in the sample they used. The result of this last step completed the estimation, and the matches between the theoretical and actual moments they reported are the only reported 'test' of the model's ability to 'fit' these series. (Lucas, 1987, pp. 42-43)

Regarding the success of their procedure, Kydland and Prescott conclude that "the fit is surprisingly good in light of the model's simplicity and the small number of free parameters" (Kydland and Prescott, 1982, p. 1345).

Despite the advantages of the Kydland-Prescott procedure,[16] it is far from conclusive as an empirical test of the time-to-build propagation mechanism (nor, it is easy to argue, is it intended to be). A major problem is the absence of a statistical approach in the test. Kydland and Prescott state that "we chose not to test our model versus the less restrictive vector autoregressive model. This most likely would have resulted in the model being rejected, given the measurement problems and the abstract nature of the model" (Kydland and Prescott, 1982, p. 1360). In fact, the methods of classical hypothesis testing are not used at all in the analysis, making it difficult to evaluate the strength of the evidence favoring the proposition that the model conforms to the postwar record. Thus the Kydland-Prescott work is far from being a decisive test of the time-to-build mechanism.

Kydland and Prescott attempt to carry out testing within the framework of a detailed structural model of the macroeconomy. While such an approach has considerable theoretical appeal, it is not particularly flexible. In searching for a general procedure within which empirical testing of the time-to-build propagation mechanism might fruitfully be undertaken, the possibility of an

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[16]Lucas' view is that "Kydland and Prescott have taken macroeconomic modeling into new territory, with a formulation that combines intelligible general equilibrium theory with an operational, empirical seriousness that rivals at least early versions of Keynesian macroeconometric models. . . . The Kydland and Prescott model is another in a long and honorable (though recently dormant) line of real business-cycle models. . . . But this time around, the terms of discussion are explicit and quantitative, and the relationship between theory and evidence can be (and is being) argued at an entirely different level. I would like to call this progress" (Lucas, 1987, pp. 46-47).

adaptation of Barro's reduced-form approach is worthy of consideration. Hall (1980, p. 158) has suggested that the general Barro procedures might be adapted to the study of investment, stating that

> so far as I know, there have not been any studies of investment within the reduced-form approach. (Any such results) are of course subject to the very basic criticism that they rest on the hypothesis of exogeneity . . . If monetary and expenditure policies have been motivated by something other than a desire to offset movements in the economy as they occur, then we can learn the effects of policies on investment simply by regressing investment on variables expressing the magnitudes of the policies. At the other extreme, if policies have been carefully tailored to eliminate all unwanted movements in investment, there may not be any regression relation, even though policy has profound effects on investment. Because policy has been far from perfect by any standard in the postwar period, because in any case it is clear that policy moves have been extremely timid when they were explicitly countercyclical, and because presumably it is output and employment, not investment, that is the principal target of policy, I think it is interesting to examine the reduced-form evidence for investment, even though I recognize that it is not fully convincing. (Hall, 1980, p. 158)

Hall's suggestion seems particularly appropriate when applied to the persistence controversy, since Barro's empirical results for unemployment and output are a main part of that controversy. A straightforward disaggregation of GNP and investigation of the persistence exhibited by its various components--particularly those making up gross private domestic investment--might lead to an interesting test of the time-to-build hypothesis. For example, imposing the appropriate restrictions on such reduced-form investment equations would be one possible way of determining whether the "state" variables associated with the time-to-build hypothesis are capable of accounting for any

persistence exhibited by investment in Barro-type equations. Further, the analysis within a reduced-form approach of business decisions to start multiperiod investment projects might also be informative in light of the prediction of the time-to-build mechanism that such series should not display "substantial" persistence. These possibilities will be explored in detail in Chapter 3, and the resulting empirical findings will be presented in Chapter 4.

The Inventories-Based (Blinder-Fischer) Propagation Mechanism

A second mechanism which is capable in principle of accounting for the persistent real impact of money-growth surprises has been advanced by Blinder and Fischer (1981), who emphasize "the role of inventories in the propagation of the business cycle in a model with rational expectations" (Blinder and Fischer, 1981, p. 277). Blinder and Fischer first explore the microeconomic implications of adding storable output to the typical firm's multiperiod optimization problem. Their findings then are applied to an otherwise traditional macroeconomic model in order to demonstrate the potential for inventories to generate a persistent response by real output to nominal-demand shocks.

Blinder and Fischer set up a model of the single-product firm where the firm's two key decision variables in any period are the amounts of production and sales. The addition to inventories in the period is then determined as the difference between the two decision variables. The firm faces convex costs associated both with production and with inventory carryover, and a linear demand curve which shifts randomly from period to

period. Blinder and Fischer initially assume that the firm can distinguish between "local" shocks to its demand and "global" shocks to the overall price level. The formal analysis involves dynamic programming and is outside the scope of this review. The optimal response of the typical firm to a positive relative price shock is described by the authors as follows:

> The profit-maximizing firm will respond by raising both sales and output. But the sales response is greater, so inventory carry-over falls. The intuition behind these results is straightforward once we keep in mind that the firm is operating on two margins: it is deciding how much to produce for inventories, and it is deciding how much to withdraw from inventories for sale. When the firm's relative price increases, the rewards for selling today (rather than tomorrow) are increased. But neither production costs nor the rewards for selling tomorrow (if (demand shocks are independent over time)) are affected. So the incentive to raise sales is greater than the incentive to raise output, and inventory stocks get depleted. (Blinder and Fischer, 1981, p. 288)

This analysis is immediately applicable to the case where the firm confuses relative-price shocks with "global" shocks to the price level. Under these circumstances, a (positive) monetary shock (not fully recognized as such by assumption) raises prices generally, which elicits a muted version of the response of the firm to a fully known relative price shock. Thus such a shock will generate sales in the current period in excess of production, drawing down inventories.

The resulting macroeconomic consequences of unanticipated inflation over time to the economy's output are summarized by the authors as follows:

> First, unanticipated inflation reduces the stock of inventories, as sales are increased in response to what firms regard in part as an increase in the relative price of output. Then inventories are gradually built

> back up . . . (Current-period) output is increased by current unanticipated inflation. Then in subsequent periods output is higher than it would otherwise have been, as a result of the need to rebuild depleted inventories. (Blinder and Fischer, 1981, pp. 291-292)

Thus the inventories-based propagation mechanism is based on the following reasoning. Assume that there is a positive nominal-demand shock in period t which increases the demand for firms' goods. Firms will meet the increase in demand in part by increasing production and in part by running down inventory stocks. Assuming that firms were holding inventories equal to their long-run desired level at the beginning of t (and assuming no change in that desired level), then it follows that, in subsequent periods, firms will increase production in order to rebuild their inventory stocks back to long-run desired levels. Thus a nominal-demand shock in t will generate increased production not only in t but also for several future periods. This will be the case even if there is only a single-period information lag, so that firms are aware in t+1 and subsequent periods of the true nature of the nominal-demand shock in t. The chain of events is reversed for the case of a negative nominal-demand shock in t: In t, stocks are built up above long-run equilibrium levels, and then are run down gradually in subsequent periods, implying less production in these future periods.

The key implication of the Blinder-Fischer mechanism is that the persistence of inventories can account for at least a considerable part of the persistence of real GNP and

unemployment.[17] On this interpretation, some of the missing "state" variables in a Barro-type reduced-form regression for output involve the inventory-accumulation behavior of firms. Further, it is evident that a necessary condition for the mechanism to play an empirically significant role in the generation of the business cycle is that inventories themselves display a persistent and positive response to nominal-demand shocks.

At first, there would appear to be a problem with the Blinder-Fischer mechanism when that propagation mechanism is offered as an explanation of why GNP responds with persistence to nominal-demand shocks. On average over the 1947-85 sample, changes in inventories comprise less than one percent of GNP, and it is not immediately clear how such a small component could be responsible for the bulk of GNP-persistence.[18] However, this argument wrongly associates GNP-persistence with the level of GNP, whereas the persistence question actually relates to the attempt to account for the deviations in GNP from trend. For example, the Barro empirical research (above, pp. 15-21).

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[17]Blinder and Fischer do not maintain that their propagation mechanism can account for all of the cyclical variation in real GNP. Thus they state in the first sentence of their paper: "There are doubtless many mechanisms that cooperate in producing the serial correlation of deviations of output from trend known as the 'business cycle'" (Blinder and Fischer, 1981, p. 277). Their view is rather that "a better understanding of inventory dynamics is critical to improving knowledge of what happens to the economy during business fluctuations" (Blinder and Fischer, 1981, p. 298).

[18]The average value for real GNP over the sample is $2202.1 billion, while the average value for changes in inventories is $14.9 billion, so that inventory changes average 0.68 percent of GNP over the sample.

assumes that the total variation in (the log of) GNP can be decomposed into a trend component and the deviation from the trend. It is this latter, relotively small, portion of total GNP--its "cyclical component"--that lagged demand shocks help to explain, and which therefore is the focus of attention in the present study. There is evidence that inventory chonges form an important part of the cyclical variation in GNP. Blinder reports that, over the 1959-79 period, "changes in (the deviation from trend of) inventory investment account for 37 percent of the variance of changes in (the deviation from trend of) GNP" (Blinder, 1981, p. 11), so that the potential importance of inventory fluctuations in explaining deviations from trend by GNP is substantial. Further, changes in GNP largely take the form of changes in inventory investment (the second difference of inventory stocks). For example, Blinder shows that "inventory investment typically accounts for about 70 percent of the peak-to-trough decline in real GNP during recessions" (Blinder, 1981, p. 11). Blinder and Fischer (1981) present similar data. Thus there are no grounds for ruling out an inventories-based propagation mechanism due to the fact that changes in inventories make up less than one percent of GNP.

Empirical testing of the inventories-based propagation mechanism

Unlike the case of the time-to-build propagation mechanism, some empirical research investigating the strength of the evidence favoring the inventories-based mechanism has been carried out within the framework of the Rational Expectations, reduced-form approach of Barro. The work of most direct

relevance is Demery and Duck (1984), where the persistent response of finished-goods inventory stocks to money shocks is investigated in the United Kingdom over a 1963:II-1979:II quarterly sample.[19] Demery and Duck utilize a money-growth-equation specification identical to that of Attfield, Demery, and Duck (1981a) (reviewed above, p. 29). Their inventories equation[20] utilizes a vector of natural-rate variables composed of (the log of) the one-period-lagged real wage rate, (the log of) the one-period-lagged relative price of materials and fuel purchased by manufacturing industry, the one-period-lagged real rate of return on money (a component of inventory holding costs), a time trend, and some seasonal dummy variables. To this specification they then add a current and four lagged values of unanticipated money growth. The authors find that "the estimates appear to confirm that monetary surprises exert a significantly negative effect on inventory holdings for about a year" (Demery and Duck, 1984, p. 372). This result is robust to a change from a two-stage to a joint estimation procedure. Demery and Duck also test the joint significance of lagged anticipated money by adding a current and four lagged values of anticipated money

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[19] In Attfield, Demery, and Duck (1981a, 1981b), and in Bellante, Morrell and Zardkoohi (1982) (reviewed on pp. 27-29, above), the persistence of U.K. real output in response to Barro-type measures of money shocks is established.

[20] The authors report that their dependent variable is "the real level of inventories held at the end of period t" (Demery and Duck, 1984, p. 370). In light of their results and the logarithmic transforms adopted in generating several of their natural-rate variables, one strongly suspects that their dependent variable is the log of inventories rather than the level. In the absence of a clarification, therefore, one cannot be sure exactly how to interpret their results.

growth to their specification. Their test "suggests that anticipated money {has} no significant impact on inventories over the period covered" (Demery and Duck, 1984, p. 375).

While the Demery-Duck results suggest that money shocks have a persisting impact on finished-goods inventory stocks, the authors' findings cannot be interpreted as providing evidence that a Blinder-Fischer type of effect is even partly responsible for the persistence of real output in the U.K. Such a conclusion would hove to rest on a finding that lagged shocks generate a positive and significant impact on inventories, not the negative and significant impact observed. It is unfortunate that Demery and Duck do not investigate the performance of their model using longer lags of money shocks as explanatory variables. The Blinder-Fischer mechanism implies that stocks are first run down, then built back up again to some long-run level, so that money shocks should first have negative and then, later, positive coefficients in an inventories equation. Demery and Duck may have found some evidence of this first-order negative impact, but, in the absence of a model estimated with longer lags of money shocks, it is impossible to draw definite conclusions regarding the overall impact of such shocks.

A U.S. study which can to some extent be interpreted as providing empirical evidence on the Blinder-Fischer mechanism is Haraf (1983). Over a 1959:III-1976:IV quorterly sample, Haraf estimates equations for real GNP, manufacturing employment, real industrial production, real finished-goods inventory stocks, and real unfilled orders. (Haraf estimates all equations in first-

difference form, relating the first differences of the above variables to the first differences of his natural-rate variables and to the first differences of money-shock measures, but for convenience this will be suppressed in the ensuing discussion.) As natural-rate variables, Haraf uses the one-period-lagged values of real GNP, real finished-goods inventories, and real unfilled orders in the GNP and employment equations, while he substitutes lagged real industrial production for lagged real GNP as natural-rate variables for his remaining three equations. He then adds to these specifications a current and nine lagged values (seven lags for inventories and unfilled orders) of the unanticipated money-growth measures developed by Barro and Rush (1980) (reviewed above, p. 24). For each equation, Haraf compares (using both F- and Likelihood Ratio-tests) the explanatory power of the full model with a restricted version where all lagged money-growth measures are deleted. He finds that "we cannot reject the hypothesis of no influence of lagged unanticipated money on real GNP, industrial production, manufacturing employment, and real unfilled orders once the supply adjustment process (presumed to be captured in the natural-rate variables) is taken into account" (Haraf, 1983, p. 115). The only equation for which lagged money shocks are jointly significant is the inventories equation. Here inspection of individual coefficients reveals results similar to those of Demery and Duck for the U.K.: a lag of negative and significant coefficients extending roughly from one to seven quarters in duration. Thus Haraf also finds evidence of a negative short-

term impact on inventory stocks (which is consistent with the Blinder-Fischer mechanism), without finding evidence of the longer-term positive impact that is essential if this mechanism is to account for the positive persistence of real output. Again one wonders what the effect would be if longer lags of money shocks were introduced into the model.

Haraf's results indicate that, if the lagged values of a number of important macroeconomic variables are included as explanatory variables in GNP, employment, industrial-production, and unfilled-orders equations, the statistically significant impact of lagged money shocks vanishes in these equations. This provides important evidence favoring the basic premise of the propagation-mechanism idea: In principle, it is easy to maintain that past shocks affect past "state" variables which then in turn impact on current-period macroeconomic real variables, so that past money shocks have no direct impact on current-period values of such variables. At the beginning of his paper, Haraf suggests that such a propagation mechanism ought to be associated with the highly cyclical behavior of unfilled orders and of inventories, and in this latter case refers to the Blinder-Fischer mechanism as a theoretical justification of his position. Moreover, Haraf's finding that only inventories continue to exhibit a statistically significant (although negative) response to lagged money shocks lends some support to his implied premise that inventories play a main role in the propagation of such shocks. However, the Haraf study--while supportive of the idea of a propagation mechanism in general--is not particularly informative

on the question of which (or how many) propagation mechanism(s) contribute to the generation of the persistent impact of money shocks. One can only speculate as to whether the Blinder-Fischer mechanism, or the time-to-build mechanism, or the wage-stickiness mechanism, or some combination, is responsible for the statistical significance of the lagged real variables in the Haraf equations. Sharper tests are required, not only of the explanatory power of the inventories-based propagation mechanism against the data, but also of its relative explanatory power when pitted against other mechanisms (such as time-to-build). Several such tests will be proposed and carried out in Chapters 3 and 4, below.

The Wage-Stickiness Propagation Mechanism

A third propagation mechanism capable in principle of accounting for the persistent impact of money shocks on real variables is the wage-stickiness mechanism advanced by Fischer (1977a) and Taylor (1980). The argument is based "on the existence of long-term contracts in the economy and makes the empirically reasonable assumption that economic agents contract in nominal terms for periods longer than the time it takes the monetary authority to react to changing economic circumstances" (Fischer, 1977a, p. 191). In the wage-stickiness mechanism, the contracts under consideration are labor contracts.

A considerable body of empirical evidence exists indicating that the adjustment of wages lags behind changing macroeconomic conditions. Hall and Taylor (1986, Chapter 14) state that, of the roughly 20 percent of the U.S. labor force that is unionized,

about half fall into the politically influential group involved in collective-bargaining situations where 1000 or more workers are involved in the process. Hall and Taylor state that contracts typically last three years in such cases, and go on to suggest that this group has influence out of proportion to its size because other union and nonunion workers tend to imitate the contracts negotiated by the larger groups. In any event, in the nonunion sector

> it is very common for workers who are not in unions to receive wage and salary adjustments once each year. Although there is no formal contract involved it is unlikely that this wage decision will be changed before the next scheduled adjustment period. Hence, the nominal wage rigidity is very similar to that in the union contracts. (Hall and Taylor, 1986, p. 385)

The wage-stickiness mechanism developed by Fischer takes as given the existence in the economy of the conditions described above. Given this key assumption, the demonstration of a persistent impact of money shocks is straightforward. Fischer first constructs a model where all wage contracts are single-period contracts, and he demonstrates that past shocks do not have real effects in this model. He then modifies the model to allow two-period contracts, and shows that this modification is sufficient to introduce a persistent response by real output to money shocks.

Both versions of Fischer's model consist of an Aggregate Supply equation, an Aggregate Demand equation, a wage-setting equation, a monetary-rule equation, and two equations describing the innovations in the disturbances to Aggregate Supply and Aggregate Demand. The models are highly stylized so as to

highlight the potential role of wage stickiness in affecting the behavior of output. Turning first to the model where there are only single-period contracts, assuming that workers contract to maintain a constant real wage, the wage determination equation is

$$(2\text{-}1) \qquad {}_{t-1}W_t = {}_{t-1}P_t,$$

where W is (the logarithm of) the nominal wage and P is (the logarithm of) the price level, and where ${}_{t-i}X_t$ denotes the expected value of X_t as of period t-i. The Aggregate Supply equation is

$$(2\text{-}2) \qquad Y^s_t = c + (P_t - W_t) + u_t,$$

where Y^s is the supply of output and u is a disturbance term. Setting c in Equation 2-2 to zero for convenience and substituting Equation 2-1 into Equation 2-2 yields the resulting expression for supply

$$(2\text{-}3) \qquad Y^s_t = (P_t - {}_{t-1}P_t) + u_t.$$

Demand considerations are taken into account via specification of a simple velocity equation

$$(2\text{-}4) \qquad Y_t = M_t - P_t - v_t,$$

where M is (the logarithm of) the money stock and v is a disturbance term. The two disturbances u and v are assumed to follow first-order autoregressive schemes

$$(2\text{-}5) \qquad u_t = r_1 u_{t-1} + e_t, \qquad -1 < r_1 < 1,$$

$$(2\text{-}6) \qquad v_t = r_2 v_{t-1} + n_t, \qquad -1 < r_2 < 1,$$

where e_t and n_t "are mutually and serially uncorrelated stochastic terms with expectation zero and finite variances" (Fischer, 1977a, p. 196). Finally, the money-supply process is assumed to depend on the values of past disturbances to Aggregate Demand and Aggregate Supply:

$$(2\text{-}7) \qquad M_t = a_1 u_{t-1} + a_2 u_{t-2} + \ldots + b_1 v_{t-1} + b_2 v_{t-2} + \ldots$$

Given Equations 2-1 through 2-7 and adding the assumption of Rational Expectations, Fischer derives

$$(2\text{-}8) \qquad P_t - W_t = P_t - {}_{t-1}P_t = -(1/2)(e_t + n_t).$$

Equation 2-8 indicates that, in the Fischer model with only single-period contracts, the difference between (the logs of) the price level and the wage level depends only on the current-period random disturbances e_t and n_t. Substituting Equation 2-8 into 2-3, it is clear that real output in the model does not depend on the unpredictable components of the shocks u and v which occurred in previous periods.

Fischer next amends his model to allow for multiperiod wage contracts. He assumes all labor contracts run for two periods, and that in any period half the firms are operating in the first half of such a contract, and the remaining firms in the second half. Thus for Equation 2-1 he substitutes

$$(2\text{-}1\text{-}A) \qquad {}_{t-i}W_t = {}_{t-i}P_t, \qquad i = 1,2,$$

so that

$$(2\text{-}9) \qquad W_t = (1/2)({}_{t-1}W_t + {}_{t-2}W_t).$$

The Aggregate Supply relation then becomes

$$(2\text{-}2\text{-}A) \qquad Y^S_t = (1/2)[(P_t - {}_{t-1}W_t) + (P_t - {}_{t-2}W_t)] + u_t,$$

or,

$$(2\text{-}3\text{-}A) \qquad Y^S_t = (1/2)[(P_t - {}_{t-1}P_t) + (P_t - {}_{t-2}P_t)] + u_t.$$

Combining the new Equations 2-1-A through 2-3-A with Equations 2-4 through 2-7, and again invoking the assumption of Rational Expectations, Fischer derives the following expression for real output:

$$(2\text{-}10) \qquad Y_t = (1/2)(e_t - n_t) + (1/3)[(a_1 + 2r_1)e_{t-1} + (b_1 - r_2)n_{t-1}] + r_1{}^2u_{t-2}.$$

For present purposes, the important characteristic of Equation 2-10 is the presence of n_{t-1}, the random component of v_{t-1}, on the right-hand side of that equation. The appearance of n_{t-1} means that current-period output depends on the random component of the nominal-demand disturbance in the previous period. Such a shock could consist of either a money-stock shock or a velocity shock or some combination of the two. Thus Fischer demonstrates the potential for current-period output to respond to money-stock shocks with persistence in a Rational Expectations environment where wage contracts are set for more than a single period.

While Fischer does not explicitly derive it, it is easy to show from his two-period-contracting model that

$$(2\text{-}11) \qquad P_t - W_t = -(1/2)(e_t + n_t) + (1/3)[(a_1 - r_1)e_{t-1} + (b_1 - r_2)n_{t-1}],$$

so that the difference between (the log of) the price level and (the log of) the wage level also depends on n_{t-1}. Equation 2-11 indicates that, in an environment characterized by unpredictable money-stock shocks and multiperiod wage contracting, $P_t - W_t$ will in general respond with persistence to money-stock shocks. The implication to a Barro-type reduced-form equation for output or unemployment--where money-growth shocks rather than money-stock shocks are utilized as explanatory variables, is that the expression

$$d(P_t - W_t) = dP_t - dW_t$$

ought to respond to Barro-type measures of money-growth shocks with persistence, and that the appropriate inclusion of such an expression in a Barro-type equation ought to eliminate the statistical significance of lagged money-growth shocks if the wage-stickiness mechanism is an important determinant of persistence.

In evaluating the wage-stickiness explanation for the persistent impact of money surprises on real variables, it is important to note that the wage-stickiness mechanism--alone among the three propagation mechanisms considered in this study--can be interpreted as implying a stabilizing role for monetary policy.

Returning to Equation 2-10, the presence of a_1 and b_1 in the equation for output means that current-period output does depend on monetary policy in the Fischer model. The explanation

> is that between the time the two-year contract is drawn up and the last year of operation of that contract, there is time for the monetary authority to react to new information about recent economic disturbances. Given the negotiated second-period nominal wage, the way the monetary authority reacts to disturbances will affect the real wage for the second period of the contract and thus output. (Fischer, 1977a, p. 199)

The extent to which the widespread presence of wage-stickiness implies a permanent stabilizing role for monetary policy has been vigorously debated.[21] However, it is probably noncontroversial to maintain that the wage-stickiness mechanism offers a stronger argument in favor of the proposition that anticipated monetary policy can affect real output than do either the time-to-build or the inventories-based mechanisms. Accordingly, empirical work finding an important role for the wage-stickiness mechanism in accounting for the persisting impact of nominal-demand shocks would not represent clearcut evidence favoring the policy-neutrality hypothesis, while empirical work supporting either of the other two mechanisms would be regarded in this fashion.

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[21]For example, Barro analyzes Fischer's argument and ultimately concludes that "some frequently discussed aspects of labor markets (including sticky wages) are a facade with respect to employment fluctuations" (Barro, 1977a, p. 316), a position that is disputed by Fischer in his response (Fischer, 1977b, p. 321). Fischer acknowledges that "an attempt by the monetary authority to exploit the existing structure of contracts to produce behavior far different from that envisaged when contracts were signed would likely lead to the reopening of the contracts and, if the new behavior of the monetary authority were persisted in, a new structure of contracts" (Fischer, 1977a, p. 204). However, this is different from saying that monetary policy has no systematic effect on output given long-term contracting.

On the above grounds, the question of the empirical evidence on the wage-stickiness mechanism can be regarded as having special interest. Therefore, it is somewhat surprising that no empirical research has been published which attempts to test the wage-stickiness mechanism within the general reduced-form approach of Barro. This is particularly puzzling in light of the clearcut testable implication emerging from the Fischer model; specifically, that the real effects of (positive) lagged money shocks in Barro-type equations should be associated with the tendency of such shocks to cause prices to rise relative to wages, thereby triggering a Fischer-type stimulation to output (and vice versa for a negative shock). A strategy for carrying out such a test will be presented in Chapter 3, and results of the test will be presented in Chapter 4.

The Barro Empirical Procedure: Technical Issues

Previous sections have reviewed the persistence question and have suggested the need for additional empirical evidence concerning the extent to which the persisting real effects of money shocks can be accounted for by one of three propagation mechanisms. The Barro "unanticipated money growth" empirical technique is a general approach within which such testing can be carried out. Use of the Barro technique in this context presumes that the approach is essentially correct, in the sense that it leads to accurate measures of unanticipated money growth. The explanatory power of lagged values of these measures then can be assessed under various conditions designed to yield information on the propagation-mechanism issue.

Since the empirical work reported in Chapter 4 is based on the Barro procedures, a technical review of the strengths and weaknesses of those procedures is in order. Most of the criticism directed at the technical aspects of Barro's work takes issue with the specifics of Barro's approach to distinguishing between anticipated and unanticipated changes in nominal demand. Following a review of this literature, several additional criticisms will be discussed and assessed.[22]

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[22]A vast body of literature also exists which seeks to discredit Barro's procedure in its guise as a test of the policy-neutrality hypothesis. For a number of reasons, these criticisms are outside the scope of the present study. The present study is an investigation of why lagged unanticipated money shocks affect the current values of real variables, rather than about why (or whether) anticipated money growth has such real effects. The crucial preliminary point to establish is thus not that Barro has carried out a conclusive test of the policy-neutrality hypothesis, but instead that the general Barro procedure leads to acceptable measures of money shocks.

Further, while the Barro findings, accepted at face value, strengthen the case for the policy-neutrality hypothesis, the case for the validity of that hypothesis does not rest fundamentally on the Barro empirical results. It rests rather on the theoretical developments comprising the "Rational Expectations revolution" (reviewed on pp. 8-11, above). Concerning the interpretation of these developments, there remains the challenge of accounting for the business cycle in a way that is consistent with the policy-neutrality hypothesis (pp. 11-14, above). Such a reconciliation must rest on the existence of some propagation mechanism or mechanisms of the general nature described in the last section. The investigation of the explanatory power of such mechanisms within the framework of the Barro technique does not require acceptance of the Barro empirical results on the policy-neutrality issue.

Finally, it is important to note that any attempt to interpret the Barro results as evidence favoring the policy-neutrality proposition rests on the assumption that the persistence of money shocks is not a contradiction of that proposition (pp. 21-23, above). In essence, there are two distinct criticisms of Barro's policy-neutrality test. One is based on alleged technical problems with that test. The other is based on the point that, granted that Barro's findings can be

Issues Concerning Specification of the Nominal-Demand Forecasting Equation

Barro's approach to specifying a nominal-demand forecasting equation (and thus also to specifying measures of nominal-demand shocks) has received extensive critical attention. Important questions have been raised regarding four main issues: first, the choice of a nominal-demand (dependent) variable; second, the choice of explanatory variables in the nominal-demand forecasting equation; third, the use of future information to forecast current values of the nominal-demand variable; and, fourth, the "observational equivalence" problem.

The choice of a nominal-demand (dependent) variable

Specification of a Barro-type forecasting equation requires the choice of some nominal-demand variable, the log-annual or log-quarterly rate of change of which is then taken as the dependent variable in the forecasting equation. Several questions have been raised in the literature concerning Barro's choice of "M1" as his nominal-demand variable. Alternatives are two: first, the choice of another monetary variable; and, second, the choice of nominal GNP rather than a monetary variable.

"M1" versus "M2" or the monetary base. Concerning his choice of "M1" instead of another monetary variable, Barro (1977b, p. 108) justifies his decision on statistical grounds: His preliminary research using postwar annual U.S. data indicated

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accepted as basically correct, there still remains the problem of accounting for the persistence of shocks in a manner consistent with the policy-neutrality hypothesis. The concern of the present work is with the second of these contentions.

that a forecasting equation using the "M1" concept of the money supply substantially outperformed forecasting equations which used either "M2" or the monetary base. While theoretical justification for such a choice is not supplied by Barro, it is implicit in the long tradition in monetary theory associating the definition of the money supply with those assets used primarily by the general public for daily transactions purposes. Such an asset is expected to be closely associated with the overall level of economic activity. It is generally acknowledged that, at least for the 1946-79 United States experience--which is the period to which Barro's studies are confined--"M1" closely approximates such a concept of the money supply.

However, when adapting Barro's techniques to U.S. samples which extend into the 1980s, special problems arise which may render the use of "M1" inappropriate in a money-growth forecasting equation. Rush (1986) suggests that in such cases the monetary base be substituted for "M1" as the nominal-demand variable in the forecasting equation. Rush argues that the main advantage of using the monetary base in these circumstances is as a means of guarding against the possible endogeneity of "M1." Another consideration is the possible impact of the Depository Institutions Deregulation and Monetary Control Act of 1980, which allows commercial banks to pay competitive interest rates on their checkable deposits (the Act is discussed in detail in Board of Governors of the Federal Reserve System, 1980b). By lowering the opportunity cost to the public of its holding "M1," the Act should lower the proportion of "M1" which is held purely for

transactions purposes, thereby altering the relationship between "M1" and the overall level of economic activity. Finally, the October 1979 announced change in Federal Reserve policy towards paying greater attention to controlling "M1" and less attention to stabilizing interest rates also has been suggested as grounds for questioning the use of "M1" in samples extending past 1979 (for example, as in Evans, 1984).

The changes described above can be interpreted as bringing about a fundamental change in the structure of the "M1"-supply process, thereby causing instability in the "M1"-growth forecasting equation. Substitution of the monetary base for "M1" in the forecasting equation avoids this problem. Concerns regarding the stability of the "M1"-forecasting equation do not undermine Barro's empirical work, the most recent sample of which ends in 1978. However, the possibility of such instability cannot be ruled out when Barro's approach is used to analyze samples extending into the 1980s.

Money versus nominal GNP. Discussion to this point has centered around criteria for choice of a particular money-growth concept as the nominal-demand variable in the forecasting equation. Gordon (1980, 1982) has suggested that nominal GNP be substituted for the money supply in the forecasting equation, while Mishkin (1983, pp. 133-140) also makes some use of a forecasting equation based on nominal GNP rather than a money-supply concept. Gordon argues that a money-growth forecasting equation in essence "requires the implicit assumption that changes in velocity have no systematic effect on prices or

output, that is, that velocity is a random serially uncorrelated variable" (Gordon, 1982, p. 1105). Gordon presents empirical evidence suggesting that this assumption is inappropriate; in any event, it would seem to be a fairly strong assumption. Gordon's argument therefore implies that Barro-type empirical work using a money-growth rather than a nominal-GNP-growth forecasting equation is vulnerable to the charge that such work implicitly misspecifies the velocity relationship.[23]

In defense of a Barro-type procedure using money shocks: While it is easy to see that taking into account velocity shocks as well as money-growth shocks is superior in principle to a setup seeking to measure only money shocks, serious questions can be raised regarding whether a forecasting equation can be constructed which effectively captures agents' attempts to forecast velocity. Conceptual problems exist for a nominal-GNP-growth forecasting equation which are much less a problem for a money-growth forecasting equation. The assumptions that the money stock depends on a reasonably small number of readily observable variables which primarily are determined exogenously to the private sector, and that forecasters are aware of the process leading to the determination of the money stock, are

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[23]Mishkin is more skeptical of the idea of a nominal-GNP forecasting equation, writing that "we should be cautious in interpreting the results (derived using a nominal-GNP equation) because the assumptions that nominal GNP growth is exogenous and that the models are reduced forms are questionable" (Mishkin, 1983, p. 133). Such a charge also can be leveled at money-growth equations. However, it is easier to argue that the money-supply process--which is to a considerable extent determined by the monetary authority--is exogenous than it is to argue that the process determining nominal GNP--which is in part dependent on the endogenous variable velocity--is exogenous.

reasonable ones. However, it is not reasonable to carry, by straightforward analogy, the same assumptions over into the realm of MV forecasting. It is easily argued that the velocity of money does not depend on any small number of variables, exogenous or endogenous, and that there is little evidence that forecasters are successful in forecasting velocity changes. The "process" determining velocity, and thus also nominal GNP, must be regarded as being relatively unknown in comparison with the process determining the money supply. On these grounds one might well conclude that modeling velocity as a random walk (which is what happens in these kinds of tests when M and not MV is taken as the variable to be forecast) is more in line with what forecasters in the actual economy do in light of the uncertainties inherent in forecasting velocity. This counterargument thus does not deny the importance of velocity shocks, but rather denies the capacity for their accurate measurement via an adaptation of the Barro empirical procedure. Evidence of the relatively inferior performance of nominal-GNP forecasting equations in comparison with money-growth forecasting equations will be presented and discussed in Chapter 4.

The choice of explanatory variables

Selection of the set of explanatory variables in the forecasting equation is a critical decision in Barro's overall approach. Barro's choice of explanatory variables in his forecasting equation has been criticized on three important grounds. First, it has been suggested that an atheoretical specification procedure is superior to the theoretical criterion

utilized by Barro. Second, critics have maintained that an appropriate forecasting-equation specification must include all the natural-rate explanatory variables contained in the second-stage equation in the forecasting equation as well. Finally, Barro's use of contemporaneous explanatory variables in his forecasting equation has been questioned. Each of these issues will be reviewed in turn.

Theoretical versus atheoretical specification procedure. Barro justifies the specification of his money-growth forecasting equation by reference to economic theory. An alternative, adopted by Mishkin (1983), is to specify the forecasting equation using some atheoretical statistical procedure. Mishkin advocates Granger-causality tests which regress the nominal-demand variable against its lagged values plus an extensive list of other lagged macroeconomic variables. Lagged values of a particular variable then are retained in the forecasting equation only if they are jointly significant at the chosen significance level. Mishkin's view is that "this procedure has the advantage of imposing a discipline on the researcher that prevents his searching for a forecasting equation specification that yields results confirming his prior on the validity of the null hypothesis" (Mishkin, 1983, p. 22). Given the long tradition in economics of specifying relationships on the basis of theory, Mishkin's argument should be interpreted more as a justification for an alternative method of specifying the forecasting equation than as a criticism of Barro's approach. Still, the question of the robustness of

results when Mishkin's procedure is substituted for Barro's is not without interest.

Inclusion of natural-rate regressors from second-stage equation. As previously discussed, Barro's approach to specifying a forecasting equation involves including as explanatory variables only those candidate variables the presence of which can be justified using economic theory. Gordon (1982), building on the work of McCallum (1979) and Nelson (1975), has asserted that a purely theoretical criterion is inappropriate on econometric grounds. He maintains that, in order to achieve consistent estimation of the relation between nominal-demand shocks and the dependent variable in the second-stage equation, it is necessary to include, as explanatory variables in the forecasting equation, all of the natural-rate explanatory variables utilized in the second-stage equation (additional variables might then also be included on theoretical grounds). Gordon's strategem assures that the nominal-demand-shock measures are "orthogonal to the other predetermined variables in the second-stage equations" (Gordon, 1982, p. 1096), thereby ruling out the possibility that second-stage results are distorted by correlations between the natural-rate variables and the shock measures. However, it is unclear how much distortion (that is, the extent of the inconsistency) is introduced by a failure to adopt Gordon's suggested procedure. Nelson's view seems to be

that, while a theoretical problem clearly exists, its potential empirical importance is relatively minor.[24]

Inclusion of contemporaneous explanatory variables. A potential misspecification of the nominal-demand forecasting equation stems from the inclusion of contemporaneous explanatory variables in such an equation. It is, in general, difficult to argue that the forecaster has knowledge of a variable the value of which is being determined in the forecasting period. Therefore, the possibility that the forecaster is being assumed to possess more information than he can reasonably be expected to have cannot be ruled out in such a case. Gordon (1982) has stressed the importance of not including contemporaneous explanatory variables in the forecasting equation, and Barro's use of the contemporaneous explanatory variable FEDV (a measure of federal expenditures relative to normal, discussed in Appendix A) has attracted criticism from Blinder (1980), Mishkin (1983, pp. 26-27) and Pesaran (1982).

Barro has argued that it is not unreasonable to assume that agents know the contemporaneous value of FEDV, since "the principal movements in FEDV, which are dominated by changes in wartime activity, would be perceived sufficiently rapidly to

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[24]Specifically, Nelson's view is that "there are few cases in practice . . . where (a natural-rate variable) can reasonably be assumed to be uncorrelated with the regression error (in the second-stage equation). As a practical matter, however, one is likely to settle for the hope that (the natural-rate variable) is not important in the formation of expectations, in other words that the offending correlation is 'small enough'" (Nelson, 1975, p. 559).

influence (forecasts of monetary growth) without a lag" (Barro, 1981b, p. 142). However, Pesaran points out that

> even if the nominal expenditure of the federal government can be predicted exactly, Barro's FEDV variable that enters his money growth equation cannot be predicted exactly unless the aggregate price level itself can be exactly predicted by the public. Thus, although it may be reasonable to argue that as a result of budget announcements the economic agents will be in a position to predict the nominal annual rate of growth of federal expenditure accurately, to assume, as Barro does, that the real growth of federal expenditure can also be exactly anticipated by the public strains credulity. (Pesaran, 1982, p. 540)

Pesaran suggests using $FEDV^e$ rather than FEDV in the forecasting equation, where

$$FEDV^e = FEDV - 0.8DGR,$$

and where the DGR are the residuals from the government-expenditures forecasting equation

$$DG = a_0 + a_1 DG1 + a_2 LUR1,$$

where the a_i are estimated coefficients, DG is the rate of growth in t of real federal expenditures, and LUR is the unemployment-rate measure previously defined (p. 18, above). Pesaran's measure of $FEDV^e$ is best interpreted as being equal to actual FEDV minus the forecasting error stemming from the necessity of predicting FEDV on the basis of past information; in effect, Pesaran's $FEDV^e$ is formed using only past information, avoiding the problem encountered by Barro.

While this criticism of Barro's forecasting equation carries theoretical weight, its empirical significance is questionable. When carrying out this adjustment to Barro's original

unemployment-rate equation, Pesaran reports results which do not differ substantially from those of Barro. Further, in his onalysis of U.S. unemployment over the 1920-83 era, Rush (1986) implements a version of Pesaran's adjustment and reports results strongly favoring the policy-neutrality hypothesis, thereby providing odditional evidence that the presence of the contemporaneous forecasting variable FEDV is not crucial to Barro's results favoring policy neutrality. Finally, in their work with quarterly U.S. data, Barro and Rush (1980) substitute a lagged measure of FEDV for FEDV in a version of their money-growth forecasting equation, and the authors report only relatively minor differences relative to the main line of their paper. In conclusion, while including the contemporaneous value FEDV as an explanatory variable in the forecasting equation is a procedure which reasonably can be questioned on theoretical grounds, there is an absence of evidence suggesting that this inclusion raises empirically-significant problems in the Barro procedure.

Use of future information to forecast current shock values

Barro's method of generating measures of onticipated and unanticipated money growth involves using the entire somple period to estimate a money-growth forecasting equation, and then identifying, first, the predicted values of the equation with anticipated money growth and, second, the residuals with unanticipated money growth. A conceptual problem inherent in such a technique is that events of later periods are used to generate predictions of money growth in earlier periods,

meaning that individual forecasters are, in some sense, assumed to be capable of using information concerning events that have not yet occurred in making their forecasts of present events. Barro's approach has been criticized on this ground, for example by Makin (1982) and Sheehan (1985).

While it is true that, taken literally, the above assumption is unrealistic, Barro defends the use of future information in his forecasting equation by arguing that use of that information in conjunction with regression analysis constrains future information to affect present forecasts in a way which is consistent with the Rational Expectations view. He points out that

> the manner in which later observations affect earlier values of {anticipated money growth} is solely through pinning down the estimates of the coefficients in the {money-growth forecasting} equation. If individuals have information about the money growth structure beyond that conveyed in prior observations--for example, from the experiences of other countries or on theoretical grounds--then the use of the overall sample period . . . may be reasonable even for the earlier dates. (Barro, 1981b, p. 141)

Thus the forecasting equation can be interpreted as a kind of proxy for a generally understood, stable money supply process, which is expected to hold for the sample period under analysis, and knowledge of which is utilized in forming predictions of current and future money growth.

The plausibility of the above argument is enhanced by the empirical finding that, in general, results derived using Barro-type procedures tend to be robust to a change from a forecasting equation which uses future information to one which utilizes only past information. Barro (1981b, p. 142) reports results from his

own studies where he utilizes money-growth predictions formed using only past information, and reports little change in the response of output and unemployment to the revised money shocks. Makin (1982) compares the explanatory power of the Barro and Rush (1980) money-shock measures with those generated by Sheffrin (1979), who uses only past information in generating his money-growth predictions. Makin finds that the series are highly correlated and that "results are not sensitive to the particular measure of anticipated money growth employed" (Makin, 1982, p. 128). Some conflicting results are reported by Sheehan (1985), who reports rejection of the hypothesis that anticipated monetary policy does not matter when only past information is used in generating predictions, and who finds support of this hypothesis when Barro's "extreme informational assumptions about the availability of data" (Sheehan, 1985, p. 527) are adopted. However, money shocks still retain substantial explanatory power in Sheehan's work. In sum, the use of future information to generate money-growth forecasts (and shocks) in the Barro procedure is a technique which can be defended both on theoretical and empirical grounds.

The "observational equivalence" problem

Implicit in the Barro procedure is the assumption that anticipated nominal demand can be distinguished from unanticipated nominal demand by using an appropriate forecasting equation. Sargent (1976b) points out conditions under which "Classical" and "Keynesian" structural models generate reduced forms which appear to respond in the same fashion to actual money

growth, despite that in the "Classical" model money is neutral while in the "Keynesian" model money is nonneutral. In the context of Barro's procedure, this "observational equivalence" problem threatens to invalidate an essential assumption of that procedure by suggesting that it may be impossible in principle to distinguish between anticipated and unanticipated nominal-demand growth.

It is generally recognized (as, for example, in Barro, 1981a, pp. 64-66) that, in principle, restrictive assumptions exist which can be imposed on Barro-type nominal-demand forecasting equations which overcome the problem with observational equivalence. The method--adopted by Barro as well as most of his followers and critics--is to impose the restriction that certain explanatory variables in the forecasting equation affect the dependent variable in the second-stage equation only through their impact on anticipated money growth. For example, in addition to lagged values of money growth, Barro's forecasting equation includes two other nonmonetary variables: a measure of federal spending relative to normal (FEDV), and a lagged unemployment-rate measure. These variables are excluded from the real output equation, so that they are constrained to affect real output only through their potential impact on predicted money growth. While Barro does not demonstrate the validity of his identification procedure, a more detailed and rigorous statement which does give this demonstration is provided by Mishkin (1983, pp. 27-31). In sum, the "observational equivalence problem" is not a major stumbling

block with respect to the attempt to meaningfully implement Barro's empirical procedures.

Additional Issues

While most of the criticisms of Barro's procedure pertain to his measurement of nominal-demand shocks, two additional issues have been raised which are of interest in the context of the present study. First, it has been claimed that Barro's findings have been biased by his failure to include longer lags of nominal-demand shocks as explanatory variables in his second-stage equations. Second, questions have been raised concerning Barro's use of a two-step (rather than joint) estimation technique in some of his studies.

Specification of the lag length of the nominal-demand variable in the second-stage equation

A crucial issue in carrying out Barro-type empirical studies concerns the criteria by which the lag length N of the nominal-demand-shock variable in the second-stage equation is to be determined. Barro and his followers typically allow statistical significance to determine this lag length. For example, Barro (1978) and Barro and Rush (1980) set N equal to two in the (annual) output equation since DMR2 (but not DMR3) is statistically significant in each of these two studies, whereas in Barro (1981b) N is set equal to one, on the grounds that DMR2 is statistically insignificant in this later study. This approach to determining lag-length specification has been criticized by Mishkin (1983), who argues that second-stage equations should be estimated with longer lags than those

resulting from the statistical-significance criterion. Mishkin (1983, pp. 116-132) carries out a test of the policy-neutrality hypothesis for 1954-76 quarterly U.S. data. Using joint estimation techniques, a time trend as a natural-rate variable, an adjustment for fourth-order serial correlation, and a money-growth forecasting equation specified according to the atheoretical statistical procedure previously described,[25] Mishkin carries out a series of Likelihood Ratio Tests which formally compare the explanatory power of anticipated versus unanticipated money growth. He concludes that

> when the lag length on unanticipated and anticipated money growth is only seven, the lag length used by Barro and Rush (1980), the likelihood ratio tests are not unfavorable to the (policy-neutrality) hypothesis. The joint hypothesis of neutrality and rationality is not rejected at the five percent level in either the output or unemployment models. . . . However, when the lag length is allowed to be longer--up to twenty lags . . .--strong rejections of the (policy-neutrality) hypothesis occur. (Mishkin, 1983, p. 116)

A remaining question concerns the appropriateness of adding explanatory variables--the longer lags of shocks--which may not belong in the regression equation on theoretical grounds. Since "including irrelevant variables will at worst only reduce the power of tests and make rejections even more telling" (Mishkin, 1983, p. 116), Mishkin concludes that his findings "therefore raise questions about previous empirical evidence from shorter lag models (such as Barro's) that supports the (policy-neutrality) hypothesis" (Mishkin, 1983, p. 118). While the

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[25]Mishkin's specification procedure leads to an "M1"-forecasting equation containing four lagged values each of "M1," Treasury Bill rates, and High-Employment Budget Surpluses as explanatory variables.

present study is not directly concerned with the contraversy over how much Barro's findings can be interpreted as evidence favoring policy neutrality (note 22, above), Mishkin's results raise the more general question of whether second-stage equations should be estimated with relatively long lags of shock terms included as explanatory variables--lags in general considerably longer than what would be called for on the basis of the criterion of statistical significance.

In assessing Mishkin's argument, it is important to note that other studies have not confirmed his results. While the early studies of Barro (1977b, 1978) and Barro and Rush (1980) do not explore robustness of results to larger values for N, Barro (1981b) and Rush (1986) in more recent research have investigated whether their results favoring policy-neutrality are robust to a lengthening of the lag length of the monetary variable. Both studies yield results favoring policy-neutrality which are robust to a variation in lag lengths of the monetary variable. Thus, Mishkin's findings are far from conclusive. However, his results do suggest that work with lag lengths longer than that dictated by use of the statistical significance criterion would be of interest.

Joint versus two-step estimation

Some authors have criticized Barro's (1977b, 1978) initial tests of the policy-neutrality hypothesis due to his exclusive use in these early studies of a "two-step" method in estimating his model (an example is Mishkin, 1983, pp. 24-26). The two-step procedure estimates the forecasting equation (the "first

step") separately from the second-stage equation (the "second-step"). "Joint" estimation methods, by controst, estimate the equations simultaneously, using Full Information Maximum Likelihood, Nonlinear Least Squares, or some similar method. While, in general, joint techniques yield superior results, considerable evidence exists suggesting that Barro's results are robust to a change from two-step to joint estimation techniques. This robustness, plus the greater flexibility (when estimating large numbers of separate second-stage-equation specifications) offered by two-step procedures, justifies the continued use of this approach that is found in the literature.

A detailed comparison of the properties of joint and two-step estimators within the context set by Barro's work is carried out by Pagan (1984). Pagan demonstrates that joint estimation methods are superior in two respects. First, estimators derived using joint estimation procedures are more efficient than the analogous estimates derived using the two-step method. The efficiency gain arises because joint estimation does not ignore the fact that the nominal-demand-shock measures are measured with sampling error, whereas two-step procedures implicitly assume that these residuals are known with certainty. Second, use of two-step procedures leads to inconsistent estimates of the standard errors of second-stage-equation coefficients, meaning that hypothesis testing can lead to invalid inferences, even in large samples. Use of joint estimation generates consistent estimates of the standard errors of coefficients in the second-stage equation.

Despite advantages stemming from use of joint estimation methods, several arguments favor some continued use of two-step procedures. As recognized by Leiderman (1980), Barro (1981b), Mishkin (1983), Pagan (1984), Murphy and Topel (1985), Rush (1986), and others, two-step estimators do yield consistent estimates of the second-stage equation's coefficients (if conditions required for reduced-form estimation are met). Second, as is pointed out by Rush (1986, p. 262), Murphy and Topel (1985, p. 370), and others, use of joint procedures entails a loss of flexibility which must be weighed against the gains in precision of inference discussed above. Further, available evidence suggests that the magnitude of error introduced by use of two-step procedures is not substantial. With respect to actual tests of the policy-neutrality hypothesis carried out to date by Barro and his followers, little difference is observed between results obtained using two-step and joint techniques. Barro (1981b), Barro and Rush (1980), Rush (1986), Leiderman (1980), Evans (1984), Mishkin (1983), and Attfield, Demery, and Duck (1981a) all are studies in which joint estimation reveals results not substantially different from analogous work using two-step estimation procedures. By contrast, there is no case in the literature where a verdict for or against policy-neutrality has been reversed by a conversion from two-step to joint estimation procedures (or vice versa).

In sum, three main points emerge from a comparison of two-step with joint estimation techniques. First, while joint estimation is the superior procedure, superiority is gained at

the cost of a substantial loss in flexibility when seeking to carry out a number of separate estimations or when seeking to impose a number of different restrictions on equations. Second, empirical work carried out on the Barro pattern leads to results which typically are the same regardless of whether joint or two-step procedures are adopted. Third, while two-step techniques are acceptable, some investigation of the robustness of results derived using two-step procedures to a switch to joint estimation techniques seems desirable in light of the superior theoretical properties of estimates stemming from use of this latter method.

Conclusion: An Assessment of the Barro Procedure

The Barro empirical procedure has encountered substantial criticism in the literature. However, most of the critics have not maintained that the procedure is fatally flawed, and the arguments of those who have--the critics of the use of "future information" in the forecasting equation, and the critics who maintain that the "observational equivalence" problem is insurmountable--seem to have been effectively met by defenders of the Barro technique. The important arguments to take into account for present purposes are those which maintain that, for one reason or another, the Barro procedure leads to a misspecification of nominal-demand-shock measures. The simplest way to meet these various objections is by exploring the robustness of any results derived in the empirical work reported below to variations in shock concept, where such variations are carried out along lines recommended by the critics of Barro. This procedure will be adopted below in Chapter 4.

CHAPTER 3
HYPOTHESES AND EMPIRICAL TESTING

In the previous chapter, evidence was presented in support of the proposition that real output responds with persistence to nominal-demand shocks. Three theories of the process of propagation also were presented, where each propagation mechanism, in principle, can reconcile the persistent response of real output with the hypothesis of Rational Expectations. The problem to be undertaken in the present chapter is twofold. First, it is to derive the relevant implications of each of the three propagation mechanisms. The second objective is to devise a series of testable hypotheses the investigation of which will allow the determination of the extent to which the propositions implied by each mechanism are consistent with the data.

Two underlying working principles are utilized in deriving testable hypotheses from the logic of the propagation mechanisms. The first is an emphasis on disaggregation, the idea being that disaggregation of the GNP accounts may lead to restrictions which in principle can distinguish between the three propagation mechanisms (even though restricting GNP itself is unlikely to lead to such discriminating tests). The second is a focus on data series closely related to but not part of the GNP accounts (such as inventory stocks and decisions by investors to start multiperiod capital-construction projects). While these

series are not part of GNP, their behavior may yield important information about the process or processes responsible for the persistence of GNP.

Implications of the Propagation Mechanisms

This section develops the implications stemming from the three propagation mechanisms under analysis: the time-to-build mechanism of Kydland and Prescott (1982), the inventories-based mechanism of Blinder and Fischer (1981), and the wage-stickiness mechanism of Fischer (1977a).

The Time-To-Build Propagation Mechanism

The time-to-build propagation mechanism has been examined in detail in Chapter 2. The mechanism implies that the duration of the persistence exhibited by real variables in response to nominal-demand shocks depends on the length of the production period for investment goods, while the pattern of this persistence depends on the period-by-period rate of progress toward completion of projects. Time-to-build[1] implies a number of propositions about the behavior of disaggregated GNP and other series closely related to GNP. Each of these implications will be developed and discussed in turn. Testable hypotheses stemming from these propositions will be put forward later in the chapter, after propositions stemming from other propagation mechanisms have been developed.

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[1]In much of the ensuing discussion, it will be convenient to abbreviate the phrase "time-to-build propagation mechanism" simply as "time-to-build."

Implications stemming from the disaggregation of GNP

All propagation mechanisms to be examined in this work are capable (in principle) of accounting for the persistent response of real GNP to nominal-demand shocks. However, this does not necessarily imply that all three also are consistent with the behavior of the disaggregated components of GNP. Therefore, it is reasonable to begin the search for testable restrictions by asking whether time-to-build (and, later, the other two propagation mechanisms) implies anything specific about the response to nominal-demand shocks of the various components of real GNP. This question will be asked about the following: first, the major components of GNP; second, the components of investment; and, third, the various subcategories making up the noninvestment components of GNP.

Disaggregation of GNP into its major components. A key implication of the time-to-build propagation mechanism is the idea that the length of the production period for a particular type of output ought to determine the degree of persistence exhibited by that output category. If different categories of GNP have widely different production periods, then a reasonable initial strategy is to disaggregate GNP into consumption, investment, government expenditures, and net exports, and investigate whether the various product categories respond with degrees of persistence consistent with what is known about their average production periods. Categories which tend on average to have longer production periods ought to be those categories exhibiting the most persistence in response to nominal-demand

shocks. Conversely, categories of output which tend on average to have short production periods should exhibit little or no persistence in response to nominal-demand shocks.[2]

The above reasoning suggests the need for knowledge of the average length of production periods for the output categories listed above. If a category has an average production period of "trivial" length--meaning that its average production period is, for annual data, under a year in length, and, for quarterly data, under a quarter in length--then that category would be expected to display minimal persistence. A category with a "nontrivial" production period would have an average production period longer than that described above, and would be subject to time-to-build in proportion to the average duration of the period of production.[3]

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[2]This latter statement ignores the possibility of interrelationships existing between an output category for which time-to-build generates substantial persistence and some other category. For example, suppose that a shock in t causes an increase in starts of new restaurants in t. In turn, new restaurants being completed and becoming productive in t+j might generate a rise in consumer food expenditures in t+j, thus tending to create a persistent response of consumers' expenditures on nondurables in t+j to the demand shock which occurred in t, even though production periods for food are very short on average. In general, detailed consideration is not given to such possibilities in the present study. However, the possibility of an empirically significant complementary relationship between producers' durable equipment and nonresidential structures will be systematically explored below.

[3]Strictly speaking, variance also would have to be taken into account. For example, if the average production period were 10 months, but 33 percent of the category were composed of goods averaging a production period of over a year in length, then the annual data might show single-period persistence without violating the restriction. There are similar ambiguous ranges for several restrictions developed in this section. However, such ambiguities will not be a problem unless the empirical results fall into the ranges of ambiguity.

Ideally, one would like to have data on average production periods for all the major categories of goods making up GNP, as well as for a number of goods within each category. Such data exist to some extent for investment (and are utilized below), but apparently do not exist for other GNP-categories. While the lack of data is to be regretted, it is a serious problem only if there are grounds for presuming that nontrivial production periods predominate for consumption, government expenditures, and net exports.

In fact, the opposite is true. First, for all categories, one expects that the overwhelming majority of goods with nontrivial production periods will be found in the durable-goods portion of the categories. Goods with production periods of substantial length tend to be complex products involving several separate and time-consuming production stages, therefore typically requiring for their production the expenditure of more resources than do goods with short production periods. Typically the purchaser, who pays the cost of production, will be able to gain more from his ownership of the item than he has expended only if he can collect a stream of utility over a considerable period of time; that is, only if the item is a durable good.[4] The empirical evidence supports this reasoning. Tables 3-1 and 3-2 give a detailed account of the types of products making up, respectively, personal consumption expenditures and government expenditures (the remaining

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[4]There are exceptions, such as fine wines and short-run Broadway shows. But the empirical importance of such items is minimal.

TABLE 3-1: TYPES OF PRODUCTS MAKING UP PERSONAL CONSUMPTION EXPENDITURES

services

housing (rentals).

household operation (electricity, gas, water and other sanitary services, telephone and telegraph, domestic service, other).

transportation (user operated, purchased local--transit systems, purchased local--other, purchased intercity).

medical care (physicians, dentists, other).

other (personal care, personal business, recreation, religious and welfare activities, net foreign travel).

nondurables

food (purchased for off-premise consumption, purchased meals and beverages, furnished to employees, consumed on farms).

clothing and shoes.

gasoline, oil, fuel oil, and coal.

other (tobacco products, toilet articles, semidurable house furnishings, miscellaneous household supplies and paper products, drug preparations and sundries, nondurable toys and sport supplies, stationery and writing supplies, net foreign remittances, other).

durables

motor vehicles and parts (new autos, net purchases of used autos, other motor vehicles, tires, tubes, accessories, and other parts).

furniture (furniture, kitchen and other household appliances, glassware and utensils, radio and television receivers, records, musical instruments, other durable house furnishings).

other (ophthalmic products and orthopedic appliances, wheel goods, durable toys, sports equipment, boats, pleasure aircraft, jewelry and watches, books and maps).

Source: U.S. Department of Commerce, National Income and Product Accounts.

TABLE 3-2: TYPES OF PRODUCTS MAKING UP GOVERNMENT EXPENDITURES

- federal
  - national defense
    - services
      - compensation of employees
      - contractual research and development
      - installation support
      - weapons support
      - personnel support
      - transportation and materiel
      - travel of persons
      - other
    - nondurable goods
      - petroleum products
      - ammunition
      - other nondurable goods
    - durable goods
      - aircraft
      - missiles
      - ships
      - vehicles
      - electronic equipment
      - other
    - structures
  - nondefense
    - services
      - compensation of employees
      - other
    - nondurable goods
    - durable goods
    - structures
- state and local
  - services
    - compensation of employees
    - other
  - nondurable goods
  - durable goods
  - structures

Source: U.S. Department of Commerce, National Income and Product Accounts.

noninvestment category, net exports, is treated separately below). It is immediately obvious from Table 3-1 that nearly all of the items making up consumer services and nondurables hove production periods well under a quarter in duration (thot is, have trivial production periods). It is more difficult to drow an analogous conclusion from Table 3-2 for government expenditures on services and nondurables, because the table supplies less detail about the types of goods making up these subcategories. Still, with the probable exception of "contractuol research and development," it seems unlikely that either the services and nondurable goods listed under notional defense spending, or the analogous items likely to make up nondefense spending, would tend to have production periods over a quarter in length. On these grounds, then, it confidently can be presumed that almost all consumer and government goods with periods of production over a quarter in duration are to be found in the durable-goods category (or, in the case of government expenditures, either in durable goods or in structures).

The above reasoning suggests that the proportion of each major category of GNP composed of durable goods might be taken as a measure of the extent to which each category is susceptible to the time-to-build effect. Table 3-3 presents summary statistics, for each major category of GNP, of the percentage of the category made up of durable items (for U.S. data over the 1947-85 period). While over a quarter of GNP is composed of durable items, only about 11 percent of personal consumption expenditures consists of durable goods. In sharp contrast, virtually the whole of gross

TABLE 3-3: "DURABILITY" OF REAL GNP AND ITS MAJOR COMPONENTS, AVERAGE VALUES OVER 1947-1985 (BILLIONS OF 1982 DOLLARS)

(1) Category	(2) Average Value	(3) Average Value of Durable Component[a]	(4) Ratio of (3) to (2)
GNP	2202.1	632.9	0.287
personal consumption expenditures	1353.8	159.3	0.112
gross private domestic investment	369.0	354.1	0.961
government expenditures	486.9	111.1	0.230
exports	180.4	69.5	0.385
imports	188.0	61.1	0.278

Source: Figures calculated from U.S. Department of Commerce data published in the National Income and Product Accounts.
Note: Figures may not add to total due to rounding.
[a]The durable component for each category is derived as follows: for personal consumption expenditures, as the value of consumers' durable-goods expenditures; for gross private domestic investment, as the value of gross fixed investment; for government expenditures, as the sum of the value of government expenditures on structures plus the value of government durable-goods expenditures; for exports and imports, as the durable-goods component of each category.

private domestic investment is made up of durables (further, changes in inventories--the investment category treated as nondurable for purposes of constructing Table 3-3--is in fact composed at least 50 percent of durables, as Table 3-13, presented below, indicates). Slightly less than a quarter of all government expenditures are for durable items.

Net exports require special treatment. A nominal-demand shock in period t ought to cause both durable exports and durable imports to increase in t+j, at the assumed time of completion. However, while exports add to GNP, imports subtract from it, so that if production periods are about the same for exports and imports, then the time-to-build effect operating on the former should be offset by an analogous effect operating on the latter. In fact, Table 3-3 reveals that U.S. exports tended to be somewhat more durable than U.S. imports over the 1947-85 era. However, the discrepancy is unlikely to be large enough to generate a tendency of net exports to exhibit persistence, even if (as is unlikely) all durable items in the category do in fact have nontrivial production periods.

In evaluating Table 3-3, a further factor to consider is that the ratio of durable goods to total goods and services is a conservative estimator of the proportion of a category's components which have nontrivial production periods, because such a measure wrongly assumes that all durable items require substantial time to build. Beginning with consumption, from Table 3-1 it is fairly clear that the types of goods making up consumer durables are, in general, likely to have production

periods much less than a quarter in length. Only special types of furniture, a small proportion of boats, and some pleasure aircraft might reasonably be expected to have longer production periods. Turning to Table 3-2 and a consideration of the probable composition of the sum of government structures and government durable goods, it is clear that some of these items are likely to have production periods greater than a quarter in length, and that, therefore, government expenditures might be somewhat more likely to show persistence than consumer expenditures. However, after taking into account the fact that not all government durables have nontrivial production periods (particularly in the state-and-local category), it is probably safe to conclude that under 20 percent of all government expenditures are for items involving substantial time to build.[5] By contrast to both consumption and government expenditures, gross private domestic investment consists almost entirely of fixed investment, which in turn consists of structures--virtually all of which is composed of goods with nontrivial production periods--and producers' durable expenditures--the balance of which is probably composed of such goods (estimates of average production periods for fixed investment are discussed in detail below).

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[5]An additional factor to consider when determining the likelihood that government goods will exhibit persistence is that such goods are not produced for purposes of making a profit, so that it is not immediately apparent why a nominal-demand shock should induce governments to start more multiperiod investment projects in the first place.

The logic of the time-to-build propagation mechanism implies that those categories of GNP with longer average production periods should exhibit the most persistence (and vice versa), while Tables 3-1 to 3-3 and the accompanying discussion strongly suggest that, of the four main categories of goods making up GNP, only gross private domestic investment should have an average production period that is nontrivial in length. Taken together, these facts imply

Proposition 1: If the time-to-build propagation mechanism is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of GNP into its four main components (consumption, investment, government expenditures, and net exports) should reveal that persistence "primarily" is confined to the investment accounts.

In order to make Proposition 1 operational, it is necessary to assign a precise meaning to the qualifier "primarily" in the above statement. Here the idea is that persistence in other categories can be reconciled with the hypothesis that time-to-build is the explanation of persistence, but only if a strong case can be presented that the persistent response of the other categories is due to the indirect effects of the time-to-build mechanism. A criterion for determining whether such is the case will be presented in due course (pp. 150-153, below). At present, it is important to note that, while a test based on Proposition 1 may lead to results which are ambiguous, other plausible findings would clearly either support or contradict the

proposition. For example, finding that only investment expenditures display persistence would unambiguously support Proposition 1, while finding no persistence in investment but substantial persistence in one or more of the other categories would clearly conflict with Proposition 1. If both investment and one or more other categories were to exhibit persistence, then the crucial factor would be the strength of the evidence that persistence in categories other than investment is due to the indirect effects of the time-to-build mechanism.

Disaggregation of gross private domestic investment. While Proposition 1 is an implication of the time-to-build propagation mechanism, it also is consistent with other investment-based propagation mechanisms such as the inventories-based mechanism of Blinder and Fischer (1981). A somewhat sharper test of time-to-build can be obtained by disaggregating gross private domestic investment into nonresidential structures, residential structures, producers' durable equipment, and changes in inventories, and then investigating the persistence of these categories.[6] The information which exists about the length of average production periods for the various investment categories is sufficient to justify a prediction about what, if any, persistence should be observed for each category, as well as the

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[6]The annual National Income and Product Accounts separate each category of investment into net investment and depreciation expenditures. Accordingly, the disaggregation of the annual investment data is into five categories: depreciation expenditures, and the net values of the four categories described above, where these four categories now sum to net private domestic investment. Quarterly data separating net investment from depreciation expenditures are not available.

amount of persistence each category should exhibit relative to the other, if the time-to-build propagation mechanism is the key to explaining the persistence of real output (these somewhat weak restrictions will be strengthened in subsequent tests, discussed below).

For nonresidential structures, the Commerce Department has published periodic surveys beginning in the 1960s of monthly progress patterns for a wide variety of types of nonresidential structures (the first published survey is in U.S. Department of Commerce, 1970). From these survey data an estimate can be derived of the average annual progress pattern for nonresidential construction over the 1961-85 period (the precise procedure by which the estimates are derived is discussed in detail in Appendix B, while the complete estimate of the quarterly progress pattern is to be found in Table 3-5, below). The survey data indicate that, on average for nonresidential structures over this period, approximately 63 percent of total investment expenditures took place in the first twelve months after the date of project start (where project start is defined in the survey as occurring when the first significant expenditures take place). Approximately 30 percent of total expenditures took place in the second year following startup, approximately 6 percent took place in the third year, and the remaining 1 percent took place after the third year. These data, taken as they are, are sufficient to imply some persistence of nonresidential structures both in the quarterly and the annual data. However, in interpreting these data, a factor deserving consideration is that business decisions

to start multiperiod projects may exhibit a certain amount of persistence without unambiguously violating the hypothesis of Rational Expectations (pp. 128-129, below). Such a tendency would increase the amount of persistence exhibited by nonresidential construction. For example, if it is assumed that a two-quarter lag typically separates the date of a money shock and the date of the investment start, the survey data suggest that approximately 29 percent of total expenditures should take place in the first year following the date of the shock, approximately 54 percent in the second year, about 15 percent in the third year, with the remaining 2 percent taking place after the third year. From these figures it can be concluded that, while the precise pattern of coefficients derivable from the survey data is unclear, the data strongly imply that persistence should be present in the response of nonresidential structures to nominal-demand shocks.

The Commerce Department also has published survey data for residential structures giving the number of months from start to completion of residential investment projects (beginning with U.S. Department of Commerce, 1970). Like the nonresidential-structures survey data, they date from the 1960s. Evidence derived from these data (again, the derivation procedure is discussed in detail in Appendix B, and the complete results are presented below in Table 3-5) indicates that, on average over the 1964-85 period, approximately 87 percent of all residential construction projects were completed within a year of their start. It can be presumed that the average lag separating the date of the decision to invest from the date of start is fairly

short in this category, due to the lesser complexity of residential construction projects (in fact, the bulk of residential construction is composed of single-unit structures). Therefore the implication is that, for annual data, residential construction should not respond with persistence to money shocks (although the contemporaneous shock term should be significant). For quarterly data, some short-term persistence should be observed, but not extending past four quarters in duration.

Estimates of average production periods for the goods making up producers' durable equipment are unavailable. Some data on order-delivery lags exist for a number of these items. They are described in detail below, where it is concluded that, judging by the likely length of production periods within the category, only moderate persistence should be displayed by producers' durable equipment both for quarterly and annual data. In this context Table 3-4, which presents the major categories making up producer durables and the average size of each in relation to the whole (over the 1947-84 period), also is informative. From Table 3-4 it can be seen that, on average over this period, almost a third of producers' durable equipment is industrial equipment, a category one might reasonably expect to be dominated by items with substantial production periods. The second largest category, other equipment (29 percent of the total), is approximately 50 percent composed of construction machinery, mining and oilfield machinery, service equipment machinery, and electrical equipment not elsewhere classified, items which also might reasonably be expected to be sufficiently complex to be

TABLE 3-4: DISAGGREGATION OF GROSS PRODUCERS' DURABLE EQUIPMENT BY MAJOR TYPE OF PRODUCT, SHOWING SHARE OF EACH COMPONENT'S CONTRIBUTION, AVERAGE VALUES OVER 1947-84 (BILLIONS OF 1982 DOLLARS)

Category	Average Value	Average Proportion of Category Making Up PDE
producers' durable equipment (PDE)	180.65	1.000
information processing and related equipment	26.14	0.136
industrial equipment	45.01	0.329
transportation and related equipment	34.68	0.245
trucks, buses, and truck trailers	14.68	0.100
autos	9.00	0.064
aircraft, ships, boats, and railroad equipment	11.00	0.081
other equipment (remaining nonresidential plus all residential)[a]	40.14	0.290

Source: Figures calculated from data supplied by the U.S. Department of Commerce, in the National Income and Product Accounts.

Note: Figures may not add to total due to rounding.

[a]Includes furniture and fixtures, tractors, agricultural machinery (except tractors), construction machinery (except tractors), mining and oilfield machinery, electrical equipment not elsewhere classified, miscellaneous other equipment, and residential equipment.

subject to a time-to-build effect. Further, the transportation category (almost 25 percent) is in part made up of aircraft, ships, boats, and railroad equipment (8.1 percent of producers' durable equipment), items for which production periods should be fairly long.[7]

In sum, the above reasoning implies

Proposition 2: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of gross private domestic investment into its constituents should produce categories which exhibit varying degrees of persistence in response to nominal-demand shocks, with the variation in persistence corresponding to what is known about the different production periods of these several categories.

Specifically, nonresidential structures ought to exhibit the most persistence for both annual and quarterly data, while more moderate persistence should be displayed by producers' durable equipment. Residential structures should exhibit persistence for quarterly data but not for annual data (although with annual data the contemporaneous shock term should be significant).

Further disaggregation of noninvestment GNP categories. When taken in conjunction with Tables 3-1 to 3-3, the time-to-build propagation mechanism implies that the main noninvestment categories of GNP should not exhibit persistence (except under the conditions discussed with respect to the interpretation of

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[7]Likely average production periods for the remaining component of gross private domestic investment--changes in inventories--will be discussed in detail below (pp. 135-139).

Proposition 1). However, even if this condition is met, there remains the possibility that a lack of a persistent response by a noninvestment category might mask substantial persistence in a portion of the category. If such a finding were for a subcategory dominated by goods with production periods of trivial length, then the result would conflict with the time-to-build hypothesis. Such considerations imply

Proposition 3: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then further disaggregation of the noninvestment components of GNP should reveal a general absence of a persistent response to nominal-demand shocks.

As was true for Proposition 1, a subcategory exhibiting persistence which has a likely average production period of trivial length will represent a weaker contradiction of time-to-build, the stronger is the case which can be presented that the persistent response of the other categories is due to the indirect effects of the time-to-build mechanism.

Subtracting fixed investment out of GNP. The time-to-build propagation mechanism, in combination with Tables 3-1 to 3-3, implies that, of the main categories of GNP, only investment expenditures should respond to nominal-demand shocks with persistence (subject to the qualifications discussed previously). One way to investigate this proposition has already been explored; specifically, to disaggregate GNP and check its components for persistence (Proposition 1). An alternative--and

for some purposes superior[8]--approach is to subtract out of GNP those categories which are produced after a relatively lengthy production period. The resulting residual is an amalgam of all those goods making up GNP which do not involve significant amounts of time to build. Its behavior should satisfy

Proposition 4: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then subtracting out of GNP those categories which are produced after a relatively lengthy production period should create a category which does not respond to nominal-demand shocks with persistence.

While an exact measurement is difficult, the information in Table 3-3 suggests that fixed investment might make a good proxy for goods with "lengthy" production periods (in fact, as has been seen above, most such goods are part of fixed investment).

Implications to the pattern of persistence of nonresidential and residential investment

The time-to-build propagation mechanism implies that the pattern of persistence exhibited by nonresidential and residential investment should be largely explained by the pace at which construction of such multiperiod projects proceeds. Since the 1960s, the Commerce Department periodically has published survey data on average rates of progress toward the completion of both nonresidential and residential construction projects that

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[8]The chief advantage of this procedure will become clear later, when a test will be developed which directly compares the explanatory power of time-to-build with that of the inventories-based propagation mechanism.

are underway during the survey period. For residential construction projects, the raw data gives the average number of months from start to completion for a wide range of residential projects. For nonresidential construction projects, the raw data gives both the number of months from start to completion as well as monthly progress patterns for a wide range of nonresidential projects. From these data, it is possible to derive estimates of the average quarterly progress patterns of both residential and nonresidential construction projects. Both the derivation procedure and the raw data are detailed in Appendix B. The resulting average quarterly progress patterns are presented in Table 3-5. Taken in conjunction with the time-to-build hypothesis, these data imply

Proposition 5: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the pattern of persistence exhibited by nonresidential and residential investment should not conflict with independent survey data giving average construction progress patterns for nonresidential and residential investment.

Implications to the pattern of persistence of producers' durable expenditures

Unlike the cases of nonresidential and residential investment, little evidence exists which yields unambiguous information on the length of production periods within the category of producers' durable expenditures. However, sufficient information exists to allow several propositions to be deduced regarding the behavior of producer durables under the assumptions

TABLE 3-5: ESTIMATED AVERAGE QUARTERLY PROGRESS PATTERNS FOR NONRESIDENTIAL AND RESIDENTIAL CONSTRUCTION PROJECTS IN QUARTERS FOLLOWING QUARTER OF START

___ Quarter Following That of Start	Proportion of Project Value Constructed for Nonresidential Projects[a]	Residential Projects[b]
0th (quarter of start)	0.1046	0.4307
1st	0.1804	0.3409
2nd	0.1867	0.1345
3rd	0.1564	0.0523
4th	0.1183	0.0270
5th	0.0808	0.0104
6th	0.0689	0.0065
7th	0.0365	0.0037
8th	0.0252	0.0018[c]
9th	0.0181	
10th	0.0104	
11th	0.0054	
12th	0.0027	
13th	0.0021	
14th	0.0013	
after 14th	0.0021	

Source: Table calculated from U.S. Department of Commerce data, published in Construction Reports, various issues. Details of sources and of computation method are given in Appendix B.

Note: Proportions may not add to one due to rounding.

[a] Surveys from which estimates were derived cover the 1961-1983 period.

[b] Surveys from which estimates were derived cover the 1964-1985 period.

[c] Estimate is for all quarters after 7th.

of time-to-build. First, data are available from which it is possible to draw tentative conclusions about average production periods within this category, which then can be used to impose restrictions on the data. Further, since nonresidential structures and producers' durable goods tend to be complements, plausible restrictions can be imposed on producers' durable expenditures on the basis of the nonresidential-construction survey data previously discussed. Finally, disaggregated annual data for producers' durable expenditures are available, and these components ought to exhibit varying degrees of persistence in accordance with what would be predicted by the time-to-build hypothesis.

Implications stemming from order-delivery lag data. The time-to-build propagation mechanism suggests that a reasonable initial approach to the problem of restricting producers' durable expenditures is to try to arrive at and utilize a measure of average production periods in the category. Survey data on production periods for the goods making up producers' durable expenditures apparently do not exist. However, several independent estimates exist of the length of the lag separating orders and deliveries for some of the goods making up the category. This "order-delivery lag" can to some extent be taken as a proxy for the production period of the item under study.

However, there is a problem inherent in an attempt to use order-delivery lags as proxies for information about production periods. To the extent that these goods already have been produced and are shipped off the shelf, or to the extent that

their production already is underway at the time of their order, an order-delivery lag will underestimate the length of the average production period, so that it also will understate the amount of persistence which should be expected in producers' durable expenditures. Therefare, when utilizing data on order-delivery lags to estimate production periods, careful attention should be given to how (or if) the data have been adjusted to compensate for a tendency toward underestimation of production periods.

Three measures of order-delivery lags for producers' durable expenditures are utilized in this study. First, the Commerce Department has published estimates, for the 1968-72 period, of order-delivery lags for the main categories of producer durables (Rottenberg and Donahoe, 1975), and the findings may cautiously be applied to the full postwar sample utilized in the present study. The Rottenberg-Donahoe results are reported in Table 3-6. They indicate that order-delivery lags for producer durables average substantially less than a year in length. If order-delivery lags can be taken as proxies for average production periods, then these estimates suggest that production periods in the producers' durable expenditure category also average substantially less than one year in length.

However, a number of questions can be raised about the extent to which the Rottenberg-Donahoe data yields reliable estimates of average production periods for producer durables over the postwar era. First, there is the tendency, mentioned above, for order-delivery lags to underestimate average

TABLE 3-6: AVERAGE ORDER-DELIVERY LAG IN MONTHS FOR TWENTY-ONE COMPONENTS OF PRODUCERS' DURABLE EQUIPMENT (1968-1972)

Category	Estimated Order-Delivery Lag (in months)
information processing and related equipment	
office, computing, and accounting machinery	3.0
communications equipment	5.0
scientific and engineering instruments	4.0
photographic equipment	0.5
industrial equipment	
fabricated metal products	6.0
engines and turbines	
steam engines	22.0
internal combustion engines	4.0
metalworking machinery	4.0
special industry machinery	4.0
general industrial, including materials handling equipment	4.0
electrical transmission, distribution and industrial apparatus	7.0
transportation and related equipment	
trucks, buses and truck trailers	2.0
railroad equipment	8.0
other equipment	
furniture and fixtures	
household furniture	0.5
other furniture	1.0
tractors	
farm tractors	0.5
construction tractors	0.5
agricultural machinery (except tractors)	0.5
construction, mining, and oilfield machinery	3.0
service industry machinery	2.0
other electrical equipment	1.0
miscellaneous equipment	0.5

Source: Rottenberg and Donahoe (1975).
Note: Estimates for other components of producers' durable equipment were not supplied by source.

production periods. Rottenberg and Donahoe attempt to adjust for such a bias by using what is known about inventory behavior in each category to correct their estimates of order-delivery lags. Their actual procedure is to estimate that portion of shipments which is "off the shelf" and subtract it from total shipments: Their estimate of order-delivery lags then is the ratio of unfilled orders to adjusted shipments. In carrying out their adjustment, however, they are forced to make several questionable assumptions.[9] Consequently, it is difficult to judge the degree to which their estimates overcome the tendency of order-delivery-lag data to underestimate average production periods. A second problem with using the Rottenberg-Donahoe estimates as proxies for production periods is that the authors' purpose in carrying out these estimates is to improve the procedures for deflating producers' durable expenditures, rather than to estimate average production periods. In at least one instance, Rottenberg and Donahoe arbitrarily shorten their estimated order-delivery lag because the long lag for this category (steam engines) would introduce distortions into their deflation

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[9]First, data classified by industry rather than by type of equipment is used in their adjustment procedure--data which may not be entirely comparable with the National Income and Product Accounts data. Second, their key adjustment parameter is an estimate of shelf items in 1967, which is assumed to equal one-half of the intermediate demand for an industry's output in 1967, a relationship the rationale for which is not made clear (and is not intuitively obvious). Finally, over the entire estimation period (1968-72), the estimate of shelf items is obtained as equalling shipments times the 1967 ratio of shelf items to total output; however, no rationale is given for this assumption.

procedures.[10] Other such adjustments may have taken place which the authors may not have felt obliged to report given the object of their study, but which are relevant when their results are applied as estimates of production periods. Finally, the Rottenberg-Donahoe estimates are for the 1968-72 period, which is only a small portion of the sample to be investigated in the present study.

Given the above, it would be useful to consider other estimates of order-delivery lags for producer durables before drawing conclusions about average production periods in that category. Zarnowitz (1973, Chapter 4), utilizing data on new orders and shipments, uses methods developed by the National Bureau of Economic Research to arrive at estimates of such lags. The procedure involves "dating the fluctuations in the series concerned, identifying the movements that match, and measuring the leads and lags between the corresponding turning points" (Zarnowitz, 1973, p. 129). Zarnowitz works with two sets of data: first, older data (mainly pre-World War II) for several goods falling within the producer-durables category; and, second, postwar (1948-64) data for durable-goods industries which are disaggregated by industry. Neither series is entirely satisfactory for present purposes, since the former data are both older and less comprehensive than is desired and the latter are industry rather than product data. Still, when considered

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[10] Table 3-6 presents the estimate reached by the authors prior to their downward adjustment.

alongside the Rottenberg-Donahoe estimates, the Zarnowitz results are informative.

Zarnowitz' figures are presented in Tables 3-7 and 3-8. Table 3-7 gives the number of months between turns in new orders and shipments, for nine components of producers' durable equipment. One major drawback of these data is that they generally are taken from an earlier period than are the data under analysis in the present study, while another is that only a small proportion of producers' durable equipment is represented in the sample. Nonetheless, a comparison with the results of Rosenberg and Donahoe is informative. Zarnowitz' findings support those of Rosenberg and Donahoe by suggesting that order-delivery lags, and by implication average production periods, for producers' durable equipment average under a year in length. However, Zarnowitz does find somewhat longer lags than do Rottenberg and Donahoe. To some extent these longer lags can be interpreted as evidence that order-delivery lags for producer durables are longer than is suggested by the Rottenberg-Donahoe estimates. However, the longer lags also might be attributable to either of the following: first, the likelihood that the goods in Zarnowitz' sample have long lags relative to the average for producer durables; and, second, the fact that the sample covers an earlier period (it is reasonable to expect technological change to shorten delivery lags over time).

Zarnowitz also presents data on the average lead of new orders relative to shipments for the major industry groups within the durable-goods industries, for the 1948-64 period. Table 3-8

TABLE 3-7: AVERAGE LEAD OF TURNS IN NEW ORDERS TO TURNS IN SHIPMENTS, NINE COMPONENTS OF PRODUCERS' DURABLE EQUIPMENT, VARIOUS PERIODS 1919-1955

Category	Average Lead in months		Ave. Deviation from Ave. Lead in months	
	Peaks or Troughs[a]	All Turns	Peaks or Troughs[a]	All Turns
industrial equipment				
fobricated structural steel (1926-55)	6.9 6.4	6.6	2.1 5.1	3.6
steel sheets (1919-32)	5.8 2.4	3.9	4.2 3.0	3.9
machine tools (1927-55)	10.0 2.8	6.0	6.5 3.0	4.7
woodworking machinery (1923-39)	2.8 0.4	1.6	1.4 1.3	1.7
transportation and related equipment				
railroad locomotives (1920-38)	5.7 8.7	7.3	3.2 1.2	3.9
railroad freight cars (1919-54)	9.1 5.2	7.1	6.1 3.0	4.9
railroad passenger cars (1919-55)	11.4 10.6	11.0	4.6 5.3	5.0
other equipment				
furniture (Grond Rapids district) (1923-46)	2.0 2.5	2.2	1.5 2.0	1.8
electric overhead crones (1926-45)	6.5 7.2	6.9	3.5 4.8	4.1

Source: Zarnowitz (1973, ch. 4, Table 4-1). Reprinted with the permission of the publisher.
Note: Estimates for other components of producers' durable equipment were not supplied by source.
[a]For each item, first-line entry is for peaks; second-line entry for troughs.

TABLE 3-8: AVERAGE LEAD OF TURNS IN NEW ORDERS TO TURNS IN SHIPMENTS, MAJOR INDUSTRIES, 1948-64

Category	Average Lead in months		Ave. Deviation from Ave. Lead in months	
	Peaks or Troughs[a]	All Turns	Peaks or Troughs[a]	All Turns
durable goods industries, total	5.6 4.6	5.1	3.5 3.0	3.3
primary metals[b]	3.6 4.6	4.1	2.5 3.5	3.1
fabricated metal products	2.2 4.5	3.5	1.5 3.2	2.9
electrical machinery	2.5 1.2	1.8	1.5 3.1	2.2
machinery except electrical	5.0 4.2	4.6	3.2 2.6	2.8
transportation equipment	2.2 3.8	3.0	4.6 4.2	4.2
other durable goods[c]	0.8 1.4	1.1	2.2 2.2	2.1
nondurable goods industries, total	2.0 2.0	2.0	2.8 2.8	2.8

Source: Zarnowitz (1973, ch. 4, Table 4-6). Reprinted with the permission of the publisher.

[a]For each item, first-line entry is for peaks; second-line entry for troughs.

[b]Industry's production is not part of producers' durable equipment.

[c]Includes professional and scientific instruments; lumber; furniture; stone, clay, and glass; and miscellaneous industries. Some of these industries mainly produce items which are not part of producers' durable equipment.

is a summary of his findings. All of the durable-goods industries except for "primary metals" and "other durable goods" produce goods virtually all of which are part of producers' durable equipment. The industry data are further evidence that production periods for producer durables average substantially less than a year in length. However, again the possibility must be raised of order-delivery lags being less than production periods, due to the fact that some orders are filled from inventory or from goods already being produced at the time of order (it can be argued that most of the goods produced by the durable-goods industry are "made to order" and are therefore not particularly subject to this bias (pp. 137-138, below), but some downward bias probably still exists).

Neither the Rottenberg-Donahoe nor the Zarnowitz estimates of order-delivery lags are for the period after 1972. A third source of information on average production periods within the category of producers' durable equipment is available for more recent years. Purchasing magazine publishes monthly survey data on leadtimes for 125 goods which are of interest to purchasing managers. While most of the commodities in the survey are intermediate goods, some are components of producers' durable equipment. Table 3-9 gives average leadtimes, over the 1974-84 period, for the 15 types of producer durables included in the Purchasing survey. Table 3-9 clearly indicates that, for these items, leadtimes average substantially under a year in length. Moreover, the leadtimes are sufficiently short so that even allowing for the previously-discussed tendency of delivery lags

TABLE 3-9: AVERAGE LEADTIMES (ORDER-DELIVERY LAGS), FIFTEEN COMPONENTS OF PRODUCERS' DURABLE EQUIPMENT, 1974-84

Category	Average Leadtime (weeks)
fabricated metal products	
weldments	6.9
structural steel, fabricated	7.1
cans	5.0
steel drums (shipping)	5.9
stampings	7.7
jigs & fixtures	8.6
general machining	6.4
powder metal parts	9.7
mechanical/electrical equipment and supplies	
electric motors: over 30 hp	11.0
machine tools	9.1
cutting tools	4.3
material handling equipment	
cranes & hoists	9.3
lift trucks	10.6
conveyors	9.0
lift truck batteries	5.5

Source: Purchasing (1974-84), various December issues.
Note: Estimates for other components of producers' durable equipment were not supplied by source.

to underestimate production periods will not alter this conclusion.

In sum, the available data point consistently to the conclusion that order-delivery lags for producers' durable equipment average well under a year in length. In turn, this conclusion implies that average production periods for such goods are less than a year in length--where, however, it is likely that the average production period is somewhat longer than the average order-delivery lag. Because of, first, this possible downward bias in measurement of average production periods, and, second, the possibility that a substantial minority of producer durables have production periods exceeding a year in length, the data cannot be construed as implying that producers' durable expenditures should not show a persistent response to nominal-demand shocks. However, the data do impose constraints on the extent of persistence which producer durables can exhibit while being consistent with the view that persistence results from time-consuming production processes within the category of producers' durable equipment. Specifically, the data imply

Proposition 6: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, and if the time-to-build effect is in proportion to the length of production periods in the category under study, then producers' durable expenditures should not exhibit a persistent response to nominal-demand shocks that exceeds a year in length. Further, nominal-demand shocks older than a year should not have more of an effect than do the more recent shocks.

That is, in the annual data, while producers' durable expenditures may exhibit moderate persistence in response to nominal-demand shocks, the category should not exhibit momentum--the one-period-lagged shock can be significant but should not have a larger impact than the contemporaneous shock term. In the quarterly data, momentum is permitted only in the case of shocks which occurred more recently than four quarters past.

Implications stemming from survey data on number of months from start to completion of nonresidential structures. To this point, discussion of how to restrict producer durables has proceeded under the presumption that the restrictions imposed by time-to-build are to be derived from the length of the average production period holding for producer durables. However, it is not clear that this is the appropriate line to take in light of the complementary relationship prevailing between producers' durable equipment and nonresidential structures. In cases where producer durables are to be placed in newly-constructed industrial and commercial structures, orders for producer durables typically will be timed so that the equipment arrives at the time the completion of the structure is estimated to take place. Such a presumption implies the following line of reasoning. Suppose that a nominal-demand shock in period t generates the start in t of a construction project which culminates in the production of a finished industrial structure in t+j. The owner will time his orders of equipment so that the equipment also is expected to arrive in t+j. Therefore, if it can further be assumed that a substantial proportion of the total

demand for producer durables stems from the construction of new structures, then one expects to see more expenditures on producer durables in periods when more nonresidential structures are completed, and vice versa.

The above reasoning implies that data giving the value of nonresidential structures completed in a period might be used to restrict the pattern of persistence exhibited by producers' expenditures on durable goods. Since the early 1960s, the Commerce Department has conducted surveys giving the number of months from start to completion for a wide range of nonresidential construction projects. Using the methods described in Appendix B, an estimate of the average start-to-completion pattern for nonresidential construction can be derived. The resulting figures are presented in Table 3-10. In combination with the above reasoning, they imply

Proposition 7: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the complementary relationship prevailing between producers' durable equipment and nonresidential structures implies that the pattern of persistence exhibited by producer durables should be explained by survey data giving the number of months from stort to completion of nonresidential structures.

Implications stemming from the disaggregation of producers' durable expenditures. The restrictions developed for producers' durable expenditures are less clearly appropriote than those previously developed for residential and nonresidential

TABLE 3-10: ESTIMATED PROPORTION OF TOTAL VALUE OF PROJECTS STARTED WHICH WAS COMPLETED IN ___ QUARTER FOLLOWING QUARTER OF START, NONRESIDENTIAL CONSTRUCTION PROJECTS, AVERAGES OVER 1961-83

___ Quarter Following That of Start	Average Proportion of Projects Completed
0th (quarter of start)	0.0063
1st	0.0313
2nd	0.0597
3rd	0.0876
4th	0.1164
5th	0.1183
6th	0.1108
7th	0.0961
8th	0.0908
9th	0.0704
10th	0.0566
11th	0.0438
12th	0.0325
13th	0.0213
14th	0.0145
15th	0.0119
after 15th	0.0319

Source: Table calculated from U.S. Department of Commerce data, published in Construction Reports, various issues. Details of sources and computation method are given in Appendix B.
Note: Proportions may not add to one due to rounding.

investment. For restrictions stemming from order-delivery-lag data, the extent to which such lags accurately portray average production periods is unclear. For restrictions stemming from the survey data on nonresidential investment giving the number of months from start to completion, the extent to which complementarity between producers' duroble equipment and nonresidential structures can be used to restrict producer durables is unclear. Moreover, the two restrictions conflict, the second implying considerably more persistence than the first. These facts suggest that additional investigation of producer durables would be useful.

Annual data are available in the National Income and Product Accounts in which producers' durable expenditures are disaggregated by type of product. Table 3-4 previously presented summary statistics of these data for the 1947-84 period. Restrictions on the disaggregated components of producer durables can be developed by noting that, while the two restrictions on producer durables developed previously conflict regarding the length of persistence which producer durables should display, they are in harmony in predicting which of the disaggregated categories ought to exhibit more or less persistence. For example, industrial equipment ought to show the most persistence of any category, regardless of whether that persistence is due to lengthy production periods within producer durables (industrial equipment ought to average the most time to build of the major components) or lengthy construction periods for industrial structures (the survey data suggest that industrial structures

average the longest time to build among nonresidential structures). Conversely, the "autos" category within "transportation and related equipment" should show little if any persistence, since production periods are short for autos and since one expects little or no complementarity between autos and nonresidential structures. These considerations imply

Proposition 8: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of producers' durable expenditures by type of product should reveal varying degrees of persistence, with results corresponding to what is known about, [a] the production periods in the various categories, and, [b] the nature of the complementary relationship likely to prevail between the category and nonresidential structures.

Implications to the behavior of investors' decisions to start multiperiod investment projects

The time-to-build propagation mechanism builds the case for persistence on lengthy production periods in one or more investment categories, implying as a corollary that variables without lengthy production periods should not exhibit substantial persistence (except where a complementary relationship with a variable averaging a significant amount of time to build can be invoked). In this context it is useful to consider the decisions of investors to start projects with nontrivial production periods (hereafter referred to as "starts"). Given a nominal-demand shock in period t, it is clear why a firm should respond with a decision to start a multiperiod investment project in t.

However, assuming a single-period information lag, the firm will observe the true nature of the shock in t+1. Assuming it has started a project in t, time-to-build explains why it might be optimal to complete the project in t+1 and subsequent periods. However, it does not explain why a firm should choose to start a project in t+1 when the firm has full information about events in t.

This reasoning suggests that an investigation of the response of "starts" over time to nominal-demand shocks might lead to a critical test of the time-to-build propagation mechanism. A number of such series are available. Examples are new orders for producers' durable goods, capital appropriations by manufacturing firms, and housing starts. Such series, reflecting decisions to acquire capital goods rather than the production of such goods, should not exhibit substantial persistence if time-to-build is the fundamental explanation of persistence.

While it is clear that "starts" should not display "substantial" persistence if the time-to-build propagation mechanism is the key to explaining persistence, how much persistence "starts" can show without conflicting with time-to-build is a difficult issue to resolve. Some authors (for example, Jorgenson, 1965), in their analysis of the time structure of the investment process, have emphasized the time-consuming nature of the preliminary planning required before the actual start of an investment project (such planning expenditures are usually classified as taking place prior to the moment of

"start"). Some of these planning activities presumably require contractual obligations on the part of the investing firm--for example, contracts with engineers and architects, acquisition of materials and supplies to be used in constructing the project, and arrangements with financial intermediaries who are involved in the financing of the project. For many of the projects for which such preliminary transactions are involved, one might still expect cancellation (or postponement) of the project to be optimal in $t+1$ given full knowledge of events in t. Still, it is not implausible to suppose that, for a number of projects, these expenditures would constitute a large enough proportion of the total expenditure to encourage completion, a factor which would tend to generate some short-term persistence in "starts."

Further, some evidence exists on the length of the lag separating change in the long-term determinants of investment demand and start of a project. The survey evidence of Mayer (1960) indicates that the length of this lag was two quarters in the mid-1950s, while research by Jorgenson and Stephenson (1967b) on the lag structure of the investment process indicates that the lag averaged three quarters in length over the 1947-60 interval. However, as previously has been seen (Chapter 2, p. 46), the extent to which the present study can draw on the conclusions of this research is unclear. The present context is different in that it involves, not a long-term change in the determinants of investment demand, but rather a nominal change that only temporarily generates a change in investment demand. Still, on the assumption that the findings of Mayer and Jorgenson and

Stephenson represent technological constraints on "starts" as discussed above, their findings can be applied to the present problem.

In sum, the above considerations, taken together, imply

Proposition 9: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then series reflecting business decisions to start multiperiod investment projects should not exhibit "substantial" persistence.

One- to two-quarter persistence might be exhibited by "starts" without clearly contradicting the time-to-build hypothesis. Greater amounts of persistence, however, suggest at minimum that the process propagating nominal-demand shocks is more complex than that envisioned by the simple time-to-build propagation mechanism.

Implications to the behavior of inventory stocks

A primary objective of this study is to develop implications of the time-to-build propagation mechanism which conflict with those of one of the other two mechanisms under consideration, so that a test can be developed which directly pits the explanatory power of time-to-build against that of the alternative hypotheses. The clearest implications of the inventories-based mechanism of Blinder and Fischer (1981) are those that constrain the behavior of inventory stocks. Therefore, it is informative to investigate whether time-to-build implies any restrictions on the behavior of inventory stocks which might later be shown to conflict with those of the Blinder-Fischer mechanism.

As has been the case previously, restrictions are developed by disaggregating the series under consideration and asking what the time-to-build propagation mechanism implies about the behavior of the components of the main series. Data are available disaggregating total inventory stocks by three separate criteria: first, by type of holder (into manufacturing, wholesale, retail, and other inventories); second, for manufacturing inventories only, by stage of production (into finished-goods, work-in-progress, and materials-and-supplies inventories); and, third, by durability of product (into durable and nondurable inventory for each category). Tables 3-11 and 3-12 present summary statistics on the first two types of disaggregated data (analogous material for the third type is presented later in the discussion). Table 3-11 reveals that, on average over the sample period, the largest single component of total inventories is manufacturing inventories (averaging 42.4 percent of the total), while the proportion held by middlemen--the sum of wholesalers' and retailers' inventories--averages 31.6 percent of the total over the sample period (the remaining 26 percent is composed of other nonfarm and farm inventories). Table 3-12 presents the results of disaggregating manufacturing inventories by stage of production, and reveals that, on average over the sample period, each of the three subcategories accounts for about a third of the total. Taken together, the two tables give an indication as to which components of total inventory stocks are likely to be most influential in determining the behavior of the total.

TABLE 3-11: DISAGGREGATION OF QUARTERLY BUSINESS INVENTORIES BY TYPE OF INDUSTRY, AVERAGE VALUES OVER 1958:IV-1979:III (BILLIONS OF 1982 DOLLARS)

Category	Average Value	Average Proportion of Category Making Up Inventories
inventories[a]	503.6	1.000
manufacturing	229.3	0.424
wholesale trade	77.5	0.141
retail trade	95.2	0.175
other[b]	139.6	0.260

Saurce: Figures calculated from data supplied by the U.S. Department of Commerce, in the National Income and Product Accounts.

Note: Figures may nat add ta total due to raunding.

[a]Inventaries data fram which figures are calculated are as of the end of the quarter.

[b]Category includes ather nonfarm plus farm inventaries.

TABLE 3-12: DISAGGREGATION OF QUARTERLY MANUFACTURING INVENTORIES BY STAGE OF PRODUCTION, AVERAGE VALUES OVER 1958:IV-1979:III (BILLIONS OF 1982 DOLLARS)

Category	Average Value	Average Proportion of Category Making Up Manuf. Inventories
manufacturing inventories[a]	229.3	1.000
finished goods	75.3	0.332
work in progress	72.6	0.313
materials and supplies	81.4	0.355

Source: Figures calculated from data supplied by the U.S. Department of Commerce.

Note: Figures may not add to total due to rounding.

[a]Inventories data from which figures are calculated are as of the end of the quarter.

All restrictions implied by time-to-build on the persistence pattern displayed by the disaggregated data ultimately are based on the following reasoning. Suppose that a nominal-demand shock in period t generates the start of production of producers' durable equipment at the beginning of t+i, where this equipment will be completed at the end of t+j. This activity is captured in the National Income and Product Accounts as follows. In the j-i intermediate periods separating the start of production and the completion of the equipment, the value of unfinished equipment will be counted as work-in-progress inventories, so that over this interval work-in-progress inventories should be increasing. At the end of t+j, the completed equipment is either shipped to a final user, in which case it is counted as producers' durable equipment, or else it is placed in inventories and counted as finished-goods inventories.

This variant of the time-to-build propagation mechanism immediately implies

Proposition 10: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in response to nominal-demand shocks, then that portion of manufacturing inventory stocks which is classified as "work-in-progress" inventories should respond to nominal-demand shocks with pronounced persistence.

In addition to the theoretical argument summarized above, some empirical support for Proposition 10 has been supplied recently in Reagan and Sheehan (1985). Using time-series analysis, the

authors analyze the behavior of manufacturers' inventories, and find that work-in-progress inventories behave noticeably more cyclically than do either finished-goods or materials-and-supplies inventories. Such a finding is consistent with Proposition 10.

A second implication of time-to-build as applied to inventories concerns the behavior of finished-goods inventories, the total of which is held by retailers, wholesalers, and manufacturers. From the discussion above, time-to-build implies that inventories of finished goods should exhibit persistence to the extent that such inventories are composed of goods with nontrivial production periods. While data are not available on the average lengths of these production periods, it is reasonable to assume that the vast majority of those goods with nontrivial production periods making up the total stock of inventories are classified as durable goods.[11] Consequently, an indication of the extent to which finished-goods inventories are subject to time-to-build can be arrived at from Table 3-13, which gives average ratios of durable to total goods for the major inventory categories. As might be expected on the basis of previous discussion, the most durable component of inventories is work-in-progress manufacturing inventories (82.2 percent composed of durable goods). Overall, about 58 percent of the sum of manufacturing, wholesale, and retail inventories are composed of durable items. Finished-goods manufacturing inventories (49.6

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[11]The rationale for this assumption is developed in detail above (pp. 94-100).

TABLE 3-13: "DURABILITY" OF INVENTORIES AND MAJOR COMPONENTS, AVERAGE QUARTERLY VALUES OVER 1958:IV-1979:III (BILLIONS OF 1982 DOLLARS)

(1) Category	(2) Average Value	(3) Average Value of Durable-Goods Component	(4) Ratio of (3) to (2)
manufacturing + wholesale + retail inventories	402.0	232.8	0.573
manufacturing	229.3	143.2	0.620
finished goods	75.3	37.4	0.496
work in progress	72.6	59.8	0.822
materials and supplies	81.4	46.0	0.558
wholesale	77.5	48.9	0.623
retail	95.2	40.7	0.416

Source: Figures calculated from data supplied by the U.S. Department of Commerce.
Note: Figures may not add to total due to rounding.

percent composed of durable items) and retail inventories (41.6 percent) are somewhat less durable than are inventories overall--retail inventories substantially so--while wholesale inventories (62.3 percent) tend to be somewhat more durable than average. Finally, materials-and-supplies manufacturing inventories (55.8 percent) are about as durable as total inventories.

The above data suggest that some persistence should be exhibited by several of the inventory categories in addition to work-in-progress manufacturing inventories if the time-to-build propagation mechanism is a primary cause of any persistence exhibited by inventories. In fact, use of the ratio of durable items to total items as a measure of a category's susceptibility to the time-to-build effect tends to overstate the case for persistence somewhat, since it is likely that many of those durable items which typically are placed in inventories do not have production periods involving any substantial amount of time to build. The argument supporting this conclusion is due to Zarnowitz (1973, Chapter 2). Zarnowitz divides industrial production into two types of goods: those made "to order" and those made "to stock." Goods manufactured "to order" are goods which fall into one of three categories:

> (1) The product must precisely meet individual consumer specifications that are virtually unpredictable; (2) the product is, in its finished form, physically or economically perishable, even though it is made from materials that are durable; (3) the product has an extremely unstable or sporadic demand, which is very difficult to forecast. (Zarnowitz, 1973, p. 11)

Within the first category fall goods such as complex machinery and fabricated steel products, which must be manufactured

according to the precise specifications of the customer. Included in the second category are nondurables such as fashion-sensitive consumer goods and certain chemicals. The third category is comprised mainly of products such as ships, locomotives, and other items which, while made to an approximately uniform specificotion, have such a high per-unit cost of production that it is prohibitively expensive to produce them in advance of an actual order. Many, if not most, of the goods classified as producers' durable equipment are manufactured "to order." By contrast, goods manufactured "to stock" are items which can profitably be produced and inventoried for later sale. Such goods tend to be items which can be manufactured in bulk to a uniform specification. While some producer durables (autos, for example) fall into this category, many--quite likely a substantial majority--do not, while one would suspect that virtually all nondurable manufactured items are made "to stock."

The above reasoning suggests that many durable goods which are made "to order"--that is, many with production periods of nontrivial length--would not normally be major components of finished-goods inventories. Such goods typically would not be produced until their order and would be promptly shipped upon completion directly to the final user, so that they would not be likely to be part of finished-goods monufacturing inventories for any considerable amount of time. Further, given that the production of many producer durables which are made "to order" require that the manufacturer follow specific and detailed instructions regarding production of the item, it is unlikely

that many such goods would be purchased through middlemen on any considerable scale, so that such goods would not typically be part of wholesale or retail inventories either.

Two separate points have been presented suggesting that finished-goods inventories ought to display less of a persistent response to nominal-demand shocks than work-in-progress manufacturing inventories. First, a lesser proportion of finished-goods inventories are composed of durable goods, a discrepancy that is largest for retail inventories. Second, finished-goods inventories are not particularly likely to be composed of durable items with substantial production periods. These considerations, taken in combination with the reasoning above, imply

Proposition 11: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in response to nominal-demand shocks, then finished-goods inventories should exhibit "substantially" less persistence than do work-in-progress manufacturing inventories. Moreover, retail inventories should display "substantially" less persistence than do wholesale or finished-goods manufacturing inventories.

As also has been the case for several of the propositions developed previously, a range of ambiguity exists for Proposition 11. However, findings which could be unambiguously interpreted are easy to note. For example, if all the persistence in inventories were due to the persistence of work-in-progress inventories, that result would support the time-to-build

hypothesis. By contrast, a tendency for persistence to be concentrated in finished-goods inventories--in particular in retail inventories--would conflict with time-to-build.

A third implication of the time-to-build propagation mechanism to the behavior of inventory stocks also can be derived. It has been noted above that, to the extent that goods with nontrivial production periods are to be found in the various categories of inventory stocks, these goods should be part of the durable-goods component of the category. Therefore, to the extent that time-to-build generates persistence for a particular category, such persistence should be confined to the durable-goods component of the category. Conversely, the nondurable-goods component of the various categories should not exhibit persistence if time-to-build is responsible for persistence of the category overall. These considerations imply

Proposition 12: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in response to nominal-demand shocks, then, for a particular inventory category, the persistence exhibited by that category should be confined to the durable-goods component of that category.

The Inventories-Based (Blinder-Fischer) Propagation Mechanism

The inventories-based propagation mechanism of Blinder and Fischer (1981) has been described in detail in Chapter 2. Its fundamental implication is that the persistent real impact of nominal-demand shocks should be associated with a persistent

response of inventories to such shocks. Specific propositions can be derived regarding the behavior of, first, changes in inventories, and, second, (disaggregated) inventory stocks.

Implications stemming from the disaggregation of GNP

Like time-to-build, Blinder-Fischer[12] implies several propositions concerning the behavior of the components of GNP. Implications can be derived concerning, first, the response of inventory changes to nominal-demand shocks, and, second, the response of the other categories of GNP to such shocks.

Changes in inventories. Previous empirical studies of the inventories-based propagation mechanism (Haraf, 1983, Demery and Duck, 1984) have related the level of inventory stocks to nominal-demand shocks (or the first differences of these series). While interesting restrictions do emerge from Blinder-Fischer on the behavior of inventory stocks (below, pp. 144-148), the behavior of changes in inventories is what is directly relevant to the problem of explaining the persistent response of GNP to nominal-demand shocks.

In Chapter 2 (above, pp. 56-57), it was argued that the issue of explaining the persistence of GNP comes down to the question of why lagged shocks can account for the deviations from trend by GNP. This interpretation of the GNP-persistence issue can be related to changes in inventories as follows. GNP (Y) can be written as

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[12]For convenience, the abbreviation "Blinder-Fischer" will be used to denote the inventories-based propagation mechanism developed by Blinder and Fischer.

$$Y = S^T + DINVY^T + s + dinvy,$$

so that

$$Y - T = s + dinvy,$$

where $T=S^T+DINVY^T$. Here S^T and $DINVY^T$ are the trends of final sales (S) and changes in inventories (DINVY), while s and dinvy are the respective deviations from these trends. The above implies that a necessary condition for Blinder-Fischer to explain why lagged nominal-demand shocks account for deviations from trend in GNP is that changes in inventories also should respond to nominal-demand shocks with persistence. Such a possibility is consistent with Blinder-Fischer: Inventory investment should be increasing over the period where firms are rebuilding stocks back to long-run levels. These considerations imply

Proposition 13: If the inventories-based propagation mechanism of Blinder and Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then changes in inventories should display pronounced persistence.

Other components of GNP. The Blinder-Fischer mechanism also imposes restrictions on those categories of GNP other than changes in inventories. Specifically, if Blinder-Fischer is correct, then "the lagged effects of unanticipated money on output that Robert Barro has found are entirely due to inventory (and unfilled orders) discrepancies caused by past unanticipated money (or, more generally, by past nominal-demand shocks)" (Blinder, 1981, p. 15). Thus Blinder-Fischer also implies

Proposition 14: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then those components of GNP other thon changes in inventories should not exhibit "substantial" persistence.

As was true for Propositions 1 and 3, other GNP-categories can display persistence without unambiguously violating Blinder-Fischer, but only to the extent that it can be argued that such persistence is the result of indirect effects of the Blinder-Fischer mechanism.

Subtracting changes in inventories out of GNP. It has been argued above that the inventories-based propagation mechanism of Blinder and Fischer implies that, of the various output categories making up GNP, only changes in inventories should display substantial persistence if Blinder-Fischer is the fundamental explanation of persistence. Propositions 13 and 14 are two implications of this hypothesis: A third can be derived by subtracting changes in inventories out of GNP and investigating the persistence exhibited by the total noninventory part of GNP. Given the arguments underlying Propositions 13 and 14, Blinder-Fischer implies that the behavior of the noninventory portion of GNP should satisfy

Proposition 15: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then subtracting inventory changes out of GNP should create an aggregate which does not respond to nominal-demand shocks with persistence.

Proposition 15 is a fairly strong restriction, which presumes that the hypothesis to be tested is that the Blinder-Fischer mechanism is the only one of importance which is at work in the data. A weaker version of the proposition recognizes the potential importance of other propagation mechanisms, and has two implications: first, that the operation described above leads to an aggregate which exhibits less persistence than does GNP; and, second, that such an aggregate should exhibit less persistence than do inventory changes considered alone. Both the strong and the weak versions of Proposition 15 will be investigated in this study.

Implications to the behavior of inventory stocks

An immediate implication of the Blinder-Fischer mechanism is that inventory stocks should respond to nominal-demand shocks with persistence. However, it has previously been seen that some persistence of inventory stocks also is implied by time-to-build. Therefore, restrictions on the behavior of total inventories are not appropriate if the objective is to devise tests which are capable of distinguishing between Blinder-Fischer and time-to-build. It is more useful to investigate whether Blinder-Fischer implies restrictions on the disaggregated inventories data described in the discussion surrounding Propositions 10 through 12.

Finished versus unfinished stocks. There is widespread agreement in the literature that the Blinder-Fischer argument is most clearly applicable to the case of finished-goods inventories. Blinder and Fischer themselves state that issues

associated with the behavior of finished-goods inventories are "the central concern of (their) paper" (Blinder and Fischer, 1981, p. 298). Demery and Duck (1984), in their theoretical and empirical study of the inventories-based propagation mechanism, express similar views.[13] Further, Blinder (1981) and Blinder and Fischer (1981) each present evidence that "investment in finished-goods inventories (mostly of retailers and wholesalers) fluctuate a good deal more than investment in other sorts of inventories" (Blinder and Fischer, 1981, p. 298).[14] These arguments imply

Proposition 16: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then finished-goods inventories--particularly retail inventories--should exhibit "substantially" more persistence than do work-in-progress and materials-and-supplies inventories.

Finding that persistence in inventories is confined to finished goods would support Blinder-Fischer, while finding that inventory persistence is confined to work-in-progress (and/or materials-and-supplies) inventories would contradict the hypothesis. A

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[13]The authors state that "this paper focuses on the propagation mechanism put forward by Blinder and Fischer (1981), who suggest that firms meet unexpected increases in demand partly by increasing production and partly by reducing inventories of finished goods. The desire by firms to replenish stocks of finished goods is the mechanism through which a current monetary surprise can influence future as well as current levels of real output" (Demery and Duck, 1984, p. 363).

[14]However, Reagan and Sheehan (1985) conclude in their empirical study (pp. 134-135, above) that input inventories account for a major portion of the variance of output.

range of ambiguity exists for the case where both finished and unfinished stocks exhibit substantial persistence, as the extent to which persistence in finished-goods inventories due to Blinder-Fischer could induce persistence in work-in-progress and/or materials-and-supplies inventories is an open question.

Pattern of finished-goods inventories persistence. The Blinder-Fischer propagation mechanism predicts that stocks of finished-goods inventories should respond to nominal-demand shocks with a special pattern of persistence. In the periods immediately following a positive shock, Blinder-Fischer holds that inventory stocks are run down as suppliers are surprised by an unexpectedly brisk demand. Then in subsequent periods inventory stocks are gradually built back up to normal levels. The reverse course of events occurs given a negative nominal-demand shock. Blinder-Fischer thus implies that the contemporaneous nominal-demand shock (and for quarterly data, perhaps also the one-period-lagged shock) ought to have an inverse, rather than a direct, effect on stocks of finished goods. This line of argument implies

Proposition 17: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the pattern of persistence exhibited by finished-goods inventories should display evidence of a short-term negative response to nominal-demand shocks.

Durable versus nondurable stocks. If Blinder-Fischer is correct, then a positive nominal-demand shock ought to cause

suppliers to first draw down and then rebuild inventory stocks generally across all categories of finished-goods inventories (and vice versa for a negative shock). In particular, the effect should be equally noticeable in durable and in nondurable stocks of finished-goods inventories. Similarly, any persistence exhibited by stocks of unfinished goods ought to be generally observable across both durable and nondurable components of the category, in order for results to be consistent with the hypothesis that the effect of Blinder-Fischer on finished-goods inventories is inducing further effects on unfinished stocks. These considerations immediately imply

Proposition 18: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of the various inventory categories into their durable and nondurable components should reveal equal amounts of persistence for durables and nondurables.

The Wage-Stickiness Propagation Mechanism

The wage-stickiness propagation mechanism of Fischer (1977a) has been reviewed in detail in Chapter 2. The argument is based on the existence of long-term labor contracts in the economy. In addition to contracts explicitly negotiated between buyers and sellers of labor, the possibility of implicit labor contracts also exists. Given the existence of such contracts, a positive nominal-demand shock causes product prices to rise faster than wages in those sectors where wages are sticky, so that the outlook for profits improves in those sectors. The result is to

induce more production than otherwise would have occurred. Conversely, a negative nominal-demand shock generates a fall in product prices relative to wages where wages are sticky, lowering profit opportunities in those sectors where wage stickiness exists, inducing less production than otherwise would have occurred.

The wage-stickiness propagation mechanism immediately implies

Proposition 19: If the wage-stickiness propagation mechanism of Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the amount of persistence exhibited by the components of GNP should be in proportion to the amount of wage-stickiness exhibited within that component.

A detailed description of the procedure by which Proposition 19 can be tested is presented below.

Testable Hypotheses

The previous section has developed 19 propositions which have been deduced from the logic of one of the three propagation mechanisms under consideration in this study. These propositions translate into 13 hypotheses that can be investigated using standard econometric techniques. (In developing these hypotheses, it will be convenient first to restate all those propositions which are relevant to the development of a particular testable hypothesis.) The hypotheses roughly are divisible into four distinct groups; specifically, into those

stemming from the following: the disaggregation of GNP, the analysis of "starts," the disaggregation of inventory stocks, and the wage-stickiness propagation mechanism.

Hypotheses Stemming from the Disaggregation of GNP

Testable hypotheses stemming from the disaggregation of GNP are of three types: first, tests of time-to-build resulting more or less directly from disaggregation; second, tests of Blinder-Fischer and of time-to-build against Blinder-Fischer; third, tests of time-to-build stemming from disaggregation in combination with independent survey data on capital-production periods.

In converting the relevant propositions of the previous section into testable hypotheses, the crucial assumption made herein is that the Barro empirical procedure (reviewed in Chapter 2, above) is as applicable to analysis of the persistence of the components of GNP as it is for a determination of the persistence of GNP itself. Thus analysis focuses on a series of reduced-form equations of the general form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn,$$

for $i=1, \ldots, k$, where

$$X_1 + X_2 + \ldots + X_k = Y.$$

DMRj is a nominal-demand shock in period t-j, b_{ij} is the coefficient relating DMRj to X_i in t, and

$$A_i = a_0 + a_1 t + a_2 C,$$

where t is a time trend, C a vector of natural-rate variables, and a_2 a corresponding vector of coefficients. Thus, the investigation of the persistence of the components of GNP will involve regressing against each of these components an appropriate vector of nominal-demand shocks and natural-rate variables. The presumption is that applying the Barro procedure to the components of GNP is informative in the sense that, from these results, one can form conclusions about the persistence exhibited by the components of GNP, and thus also conclusions about which components of GNP play the key role in propagating nominal-demand shocks. One then can compare these results with the implications of the various propagation mechanisms to determine the extent to which each is consistent with the data.

Form of dependent variables

Before proceeding as described above, the form(s) of the function $f(\cdot)$ in the above equations must be selected. The primary definition of dependent variables utilized is

$$f(X_i) = (X_i/Y)\cdot L(Y), \quad i=1, \ldots, k.$$

Thus the dependent variables are not the various components of real GNP (Y) themselves, but instead equal L(Y) weighted by the ratio of the particular subcategory to Y. One advantage of defining dependent variables in this way is that the $(X/Y)\cdot L(Y)$ sum to L(Y), which is not the case if the logs of each component are the dependent variables. A second advantage is that, unlike the case where the logs of each component are the dependent

variables, the above is defined for negative values of both changes in inventories and net exports (a third advantage is discussed below, pp. 158-160). The specification is not without its drawbacks, however, since a positive coefficient on a nominal-demand shock now indicates that this shock is causing the category to increase relative to output, rather than increasing absolutely (and vice versa). Thus for $(X/Y)\cdot L(Y)$ to be unresponsive to lagged nominal-demand shocks does not necessarily imply that X (or L(X)) would be unresponsive to such shocks. This suggests that an alternative form for dependent variables is required if a full assessment of the persistence exhibited by the various categories making up GNP is to be made. Therefore, it will be informative to supplement analysis using the above form for dependent variables with complementary analysis using the specification

$$f(X_i) = L(X_i), \text{ for } i=1, \ldots, k.$$

Where both $(X_i/Y)\cdot L(Y)$ and $L(X_i)$ show a persistent response to nominal-demand shocks, it indicates not only persistence but also suggests that this persistence has not been induced by a rise in real GNP, since X_i is rising both absolutely and relative to any rise in real GNP. Such a result would appear to be a necessary (though not a sufficient) condition in order for the process of propagation to be fundamentally related to X_i. This result will be denoted by the term "strong persistence" in

the ensuing discussion,[15] and will be regarded as evidence favoring the position that the essence of the process of propagation involves X_i.

Where $L(X_i)$ shows a persistent response to nominal-demand shocks but $(X_i/Y)\cdot L(Y)$ does not, it indicates that, while the category itself responds to nominal-demand shocks with persistence, such shocks do not cause X_i to rise relative to Y. In such circumstances, one cannot rule out the likelihood--and, given persuasive theoretical grounds, the strong possibility--that the persistence of $L(X_i)$ has been induced by the accompanying rise in real output (the alternatives would be either that both X_i and Y have risen due to the influence of a third variable, or that they have each risen independently). This result will be denoted as "weak persistence" in the ensuing discussion, and will be taken as evidence favoring the notion that the essence of the propagation mechanism does not involve X_i, although X_i ultimately contributes to the propagation of nominal-demand shocks through its response to Y.

The result where neither $(X_i/Y)\cdot L(Y)$ nor $L(X_i)$ exhibits a persistent response to nominal-demand shocks will be denoted as one of "no persistence." Finally, the (relatively rare) case where $(X_i/Y)\cdot L(Y)$ exhibits persistence but $L(X_i)$ does not, indicates a situation where Y rises or falls relative to X_i over

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[15]For changes in inventories and net exports, the L(X) form of the dependent variable is undefined, and the term "strong persistence" will be used to denote the case where the $(X/Y)\cdot L(Y)$ form of the variable exhibits a persistent response to shocks.

time in response to nominal-demand shocks, even though X_i does not respond to such shocks.

Tests of time-to-build resulting directly from the disaggregation of GNP

Propositions 1 through 3, developed above in detail, summarize those implications of the time-to-build propagation mechanism which stem directly from the disaggregation of real GNP. These three propositions translate in straightforward fashion into three testable hypotheses. The implications of time-to-build to a first-order disaggregation of GNP are given by

Proposition 1: If the time-to-build propagation mechanism is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of GNP into its four main components (consumption, investment, government expenditures, and net exports) should reveal that persistence "primarily" is confined to the investment accounts.

Proposition 1 implies

Hypothesis 1: Given four Barro-type regression equations of the form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, 4,$$

where the X_i are the four components of GNP listed in Proposition 1 and other variables have the meanings previously assigned, then, for all $j>0$: [A] Where $f(X_i)=(X_i/Y)\cdot L(Y)$, for any noninvestment category i, $b_{ij}=0$; [B] Where $f(X_i)=(X_i/Y)\cdot L(Y)$,

for the investment category i, the b_{ij} as a group are jointly significant; [C] Where $f(X_i)=L(X_i)$, for the investment category i, the b_{ij} as a group are jointly significant. That is, only investment should display "strong persistence" in response to nominal-demand shocks.

The implications stemming from the disaggregation of (the fixed-investment portion of) gross private domestic investment are given by

Proposition 2: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of gross private domestic investment into its constituents should produce categories which exhibit varying degrees of persistence in response to nominal-demand shocks, with the variation in persistence corresponding to what is known about the different production periods of these several categories.

Proposition 2 implies

Hypothesis 2: Given a series of Barro-type regression equations of the form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, k,$$

where the X_i are the components of gross private domestic investment and other variables have the meanings previously assigned, the following should hold both where $f(X_i)=(X_i/Y)\cdot L(Y)$ and where $f(X_i)=L(X_i)$. [A] For nonresidential structures and

producers' durable expenditures, for all $j>0$, the b_{ij} as a group are jointly significant. [B] For residential structures, for all $j>0$ (annual data) and for all $j>4$ (quarterly data), $b_{ij}=0$; further, for quarterly data, for all $j>0$, the b_{ij} as a group are jointly significant. Thus, [A] and [B] together imply that "strong persistence" of a form consistent with the above should be observed for all three categories. [C] Let b_{iM} be the oldest statistically significant coefficient for category i, and let M_1, M_2, and M_3 be the values of M in the equations for nonresidential structures, producers' durable expenditures, and residential structures, respectively: then $M_1>M_2>M_3$.

The implications of time-to-build to the noninvestment components of GNP are given by

Proposition 3: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then further disaggregation of the noninvestment components of GNP should reveal a general absence of a persistent response to nominal-demand shocks.

Proposition 3 implies

Hypothesis 3: Given a series of Barro-type regression equations of the form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, k,$$

where the X_i are the components making up consumption expenditures and government expenditures, and other variables have the meanings previously assigned, then where $f(X_i)=(X_i/Y)\cdot L(Y)$, for all $j>0$ and category i, $b_{ij}=0$. That is, no component of consumption or government expenditures should exhibit "strong persistence."

Tests of Blinder-Fischer and of time-to-build against Blinder-Fischer

The Blinder-Fischer mechanism also implies several propositions concerning the behavior of the disaggregated components of real GNP. The implications of Blinder-Fischer which stem from a straightforward disaggregation of GNP are given by

Proposition 13: If the inventories-based propagation mechanism of Blinder and Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then changes in inventories should display pronounced persistence;

and,

Proposition 14: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then those components of GNP other than changes in inventories should not exhibit "substantial" persistence.

Taken together, Propositions 13 and 14 imply

Hypothesis 4: Given a series of Barro-type regression equations of the form

$$(X_i/Y)\cdot L(Y) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj$$

$$+ \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, k,$$

where the X_i are components of GNP and other variables have the meanings previously assigned, then for all $j>0$: [A] For any noninventory category i, $b_{ij}=0$; [B] For X_i=changes in inventories, the b_{ij} as a group are jointly significant. That is, only changes in inventories should display "strong persistence" in response to nominal-demand shocks.

Hypotheses directly pitting the explanatory power of time-to-build against that of Blinder-Fischer stem from

Proposition 4: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then subtracting out of GNP those categories which are produced after a relatively lengthy production period should create a category which does not respond to nominal-demand shocks with persistence;

and,

Proposition 15: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then subtracting inventory changes out of GNP should create an aggregate which does not respond to nominal-demand shocks with persistence.

Two distinct tests result from Propositions 4 and 15, which will be labeled Hypotheses 5a and 5b, respectively.

Hypothesis 5a: Given two Barro-type regression equations of the form

$$L(Y-X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1,2,$$

where X_1 is (from Proposition 4) the sum of investment in structures and investment in producers' durable equipment, X_2 is (from Proposition 15) changes in inventories, and where other variables have the meanings previously assigned. Then: [A] If the process of propagation chiefly involves time-to-build, the b_{1j} are jointly significant, while, for all $j>0$, $b_{2j}=0$; [B] If the process of propagation chiefly involves Blinder-Fischer, the b_{2j} are jointly significant, while, for all $j>0$, $b_{1j}=0$.

An alternative version of Hypothesis 5 can be derived by using $[(Y-X_i)/Y]\cdot L(Y)$ as the form for the dependent variables, where the X_i are defined as for Hypothesis 5a. Additional insight as to the meaning of such a formulation of the dependent variables can be gained from the following. The regression equation for real output can be written in the form

$$L(Y) = A_y + b_0DMR + b_1DMR1 + b_2DMR2 + \ldots + b_jDMRj + \ldots + b_nDMRn,$$

where A_y is A_i (defined on p. 150, above) as applied to the L(Y) equation, and other variables have the meanings previously assigned. For a given X_i, this can be expressed as

$$[(Y-X_i)/Y]\cdot L(Y) = A_y + b_0 DMR + b_1 DMR1 + b_2 DMR2 + \ldots + b_j DMRj + \ldots + b_n DMRn - (X_i/Y)\cdot L(Y),$$

or,

$$[(Y-X_i)/Y]\cdot L(Y) = (A_y - A_i) + (b_0 - b_{i0})DMR + (b_1 - b_{i1})DMR1 + (b_2 - b_{i2})DMR2 + \ldots + (b_j - b_{ij})DMRj + \ldots + (b_n - b_{in})DMRn,$$

where A_i and the b_{ij}s stem from a regression of the natural-rate variables and the DMRs on $(X_i/Y)\cdot L(Y)$. The above makes it clear that if a coefficient $(b_j - b_{ij})$ in the expression above equals zero, then the effect of DMRj on $(X_i/Y)\cdot L(Y)$ does not differ significantly from its effect on L(Y). If $(b_j - b_{ij})$ is greater than zero, then the effect of DMRj on L(Y) is greater than its effect on $(X_i/Y)\cdot L(Y)$; that is, DMRj causes $Y-X_i$ to rise relative to Y (and vice versa for $[b_j - b_{ij}]$ negative). Similarly, if all $(b_j - b_{ij})$ equal zero, then the pattern of coefficients on $(X_i/Y)\cdot L(Y)$ is identical to the pattern of coefficients on L(Y), suggesting that all the persistence in Y is accounted for by X_i. Thus this is a somewhat stronger and more interesting test than Hypothesis 5a.

The above argument immediately implies

Hypothesis 5b: Given two Barro-type regression equations of the form

$$[(Y-X_i)/Y]\cdot L(Y) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1,2.$$

where X_1 and X_2 are defined as for Hypothesis 5a, and other variables have the meanings previously assigned. Then: [A] If the process of propagation chiefly involves time-to-build, the b_{1j} are jointly significant, while, for all $j>0$, $b_{2j}=0$; [B] If the process of propagation chiefly involves the Blinder-Fischer mechanism, the b_{2j} are jointly significant, while, for all $j>0$, $b_{1j}=0$.

Tests of time-to-build using independent survey data on production periods

Three hypotheses derived from the time-to-build hypothesis make use of the disaggregated investment-accounts data in combination with survey data on capital-construction periods. The implications stemming from use of data on average construction progress patterns for structures are summarized by

Proposition 5: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the pattern of persistence exhibited by nonresidential and residential investment should not conflict with independent survey data giving average construction progress patterns for nonresidential and residential investment.

Proposition 5 implies

Hypothesis 6: Given two Barro-type regression equations of the form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj$$

$$+ \ldots + b_{in}DMRn, \text{ for } i=1,2,$$

where $f(X_i)$ is either $(X_i/Y)\cdot L(Y)$ or $L(X_i)$, X_1 and X_2 are nonresidential and residential construction, respectively, and other variables have the meanings previously assigned. Also given average progress patterns for nonresidential and residential construction projects (from Table 3-5, above): c_{i0}, c_{i1}, . . . , c_{ij}, . . . , c_{in}, $i=1,2$. Then a regression equation of the form

$$f(X_i) = A_i + b_{i0}DMR + \ldots + b_{iu}DMRu + v[c_{i0}DMR(u+1)$$

$$+ \ldots + c_{ij}DMRj + \ldots + c_{in}DMRn],$$

where u is the longest "acceptable" lag separating "starts" of X_i from the date of the shock generating the start (pp. 128-129, above), and v is a parameter to be estimated, provides a fit which is not significantly worse than that provided by the unrestricted regression equation.

The implications stemming from survey data giving likely average production periods for producers' durable expenditures are summarized by

Proposition 6: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, and if the time-to-build effect is in proportion to the length of production periods in the category under study, then producers' durable expenditures should not exhibit a persistent response to

nominal-demand shocks that exceeds a year in length. Further, nominal-demand shocks older than a year should not have more of an effect than do the more recent shocks.

Proposition 6 implies

Hypothesis 7: Given a Barro-type regression equation of the form

$$f(PDUR) = A_{pd} + b_0 DMR + b_1 DMR1 + b_2 DMR2 + \ldots + b_j DMRj + \ldots + b_n DMRn,$$

where f(PDUR) is either $(PDUR/Y)\cdot L(Y)$ or L(PDUR), where PDUR denotes producers' durable expenditures, A_{pd} is A_i (defined on p. 150, above) as applied to the f(PDUR) equation, and other variables are as defined previously, then, for quarterly data, for all $j>4$, $b_j=0$.

The implications to the pattern of persistence exhibited by producers' durable expenditures which stem from nonresidential-structures start-to-completion survey data are summarized by

Proposition 7: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the complementary relationship prevailing between producers' durable equipment and nonresidential structures implies that the pattern of persistence exhibited by producer durables should be explained by survey data giving the number of months from start to completion of nonresidential structures.

Proposition 7 implies

Hypothesis 8: Given a Barro-type regression equation of the form

$$f(PDUR) = A_{pd} + b_0 DMR + b_1 DMR1 + b_2 DMR2 + \ldots + b_j DMRj + \ldots + b_n DMRn,$$

where f(PDUR) is either (PDUR/Y)·L(Y) or L(PDUR), where PDUR denotes producers' durable expenditures and other variables are as defined previously. Also given average start-to-completion patterns for nonresidential construction projects (from Table 3-10, above): $c_0, c_1, \ldots, c_j, \ldots, c_n$. Then a regression equation of the form

$$f(PDUR) = A_{pd} + v[c_0 DMR + c_1 DMR1 + \ldots + c_j DMRj + \ldots + c_n DMRn],$$

where v is a parameter to be estimated, provides a fit which is not significantly worse than that provided by the unrestricted regression equation.

Finally there is

Proposition 8: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of producers' durable expenditures by type of product should reveal varying degrees of persistence, with results corresponding to what is known about, [a] the production periods in the various categories, and, [b] the nature of the

complementary relationship likely to prevail between the category and nonresidential structures.

Specific hypotheses are not developed from Proposition 8, which is investigated in Chapter 4 mainly in order to supplement the work stemming from Hypotheses 7 and 8.

Hypothesis Stemming from the Analysis of "Starts"

The crucial implication which relates to investor decisions to start (or bring about the start of) the construction of capital-goods requiring considerable amounts of time to build is

Proposition 9: If time-to-build is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then series reflecting business decisions to start multiperiod investment projects should not exhibit "substantial" persistence.

Proposition 9 implies

Hypothesis 9: Given a series of Borro-type regression equations of the form

$$L(S_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, k.$$

where S_i is the ith "starts" concept utilized as a dependent variable, and other variables have the meanings defined previously. Let b_{iM} be the coefficient associated with the oldest DMR_i which can show persistence without unambiguously

clashing with the logic of time-to-build (pp. 128-129, above). Then, for all $j>M$, $b_{ij}=0$.

Hypotheses Stemming from the Disaggregation of Inventory Stocks

Six propositions previously have been derived which restrict the behavior of real inventory stocks on the basis either of the time-to-build or the Blinder-Fischer mechanisms. These six propositions in turn imply three testable hypotheses about the behavior of the components of real inventories. Hypothesis 10 stems from the disaggregation of total stocks by type of holder; specifically, from the following three propositions:

Proposition 10: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in response to nominal-demand shocks, then that portion of manufacturing inventory stocks which is classified as "work-in-progress" inventories should respond to nominal-demand shocks with pronounced persistence;

Proposition 11: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in response to nominal-demand shocks, then finished-goods inventories should exhibit "substantially" less persistence than do work-in-progress manufacturing inventories. Moreover, retail inventories should display "substantially" less persistence than do wholesale or finished-goods manufacturing inventories;

Proposition 16: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-

demand shocks, then finished-goods inventories--particularly retail inventories--should exhibit "substantially" more persistence than do work-in-progress and materials-and-supplies inventories.

Propositions 10, 11, and 16, taken together, imply

Hypothesis 10: Given a series of Barro-type regression equations of the form

$$L(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, 4,$$

where: X_1=finished-goods manufacturing inventories, X_2=work-in-progress manufacturing inventories, X_3=wholesale inventories, and X_4=retail inventories; and other variables are as previously defined. Then the following are implied.

[A1] For all $j>0$, a necessary condition for the process of propagation to be dominated by time-to-build is that the b_{2j} are jointly significant; while: [A2] For all $j>0$, a necessary condition for the process of propagation to be dominated by Blinder-Fischer is that the b_{3j} and/or the b_{4j} are jointly significant.

As an aid to developing additional tests, it is helpful to define the "Z-coefficient"

$$Z_i = (f_i/k)\cdot[(b_{i1}/B_i)\cdot 1 + (b_{i2}/B_i)\cdot 2 + \ldots + (b_{ij}/B_i)\cdot j + \ldots + (b_{iM}/B_i)\cdot M],$$

where Z_i is the Z-coefficient for inventory category i, f_i is the number of the k lagged DMR coefficients that are statistically significant for the category-i equation, b_{ij} is the statistically significant coefficient on DMRj for category i, B_i is the sum of the statistically significant coefficients on the lagged DMR for category i, and M is the last quarter following the shock for which a statistically significant response remains (out of k) for category i--that is, DMR(M) is the oldest shock to be statistically significant in the category-i equation. The Z-coefficient is thus a measure of the magnitude of persistence exhibited by $L(X_i)$: It weights persistence more highly the later after the shock the persistence occurs (but at a decreasing rate), and weights persistence more highly according to the frequency--the proportion of significant coefficients--with which it occurs. However, the coefficient does not reward a larger total effect as measured by the sum of the coefficients in a given equation. Nor does it place more weight on significant DMR coefficients with larger t-statistics.

Use of the Z-coefficient leads to the second and third parts of Hypothesis 10: [B1] If the propagation process is dominated by time-to-build, then Z_2 is greater than Z_1, Z_3, and Z_4; while: [B2] if the propagation process is dominated by Blinder-Fischer, then Z_1, Z_3 and Z_4 are greater than Z_2. And, further: [C1] If the propagation process is dominated by time-to-build, Z_4 is less than Z_1, Z_2 and Z_3; while: [C2] if the propagation process is dominated by Blinder-Fischer, Z_4 is greater than Z_1, Z_2, and Z_3.

A second testable hypothesis relates only to the Blinder-Fischer mechanism and derives from

Proposition 17: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the pattern of persistence exhibited by finished-goods inventories should display evidence of a short-term negative response to nominal-demand shocks.

Proposition 17 implies

Hypothesis 11: Given a series of Barro-type regression equations of the form

$$L(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, 3,$$

where X_1 equals, alternatively, finished-goods manufacturing inventories, wholesale inventories, or retail inventories; and other variables are as previously defined. Let the duration of the short-term inventory rundown (buildup) associated with a positive (negative) nominal-demand shock by Blinder-Fischer equal M periods. If the propagation process chiefly involves Blinder-Fischer, then, for $j<(M+1)$, $b_{ij}<0$.

A final testable restriction on the behavior of inventory stocks stems from

Proposition 12: If time-to-build is the fundamental explanation of any persistent response exhibited by inventory stocks in

response to nominal-demand shocks, then, for a particular inventory category, the persistence exhibited by that category should be confined to the durable-goods component of that category;

and

Proposition 18: If Blinder-Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then disaggregation of the various inventory categories into their durable and nondurable components should reveal equal amounts of persistence for durables and nondurables.

Taken together, Propositions 12 and 18 imply

Hypothesis 12: Given a series of Barro-type regression equations of the form

$$L(Xd_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, 4,$$

and

$$L(X_i - Xd_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, 4,$$

where: X_1=finished-goods manufacturing inventories, X_2=work-in-progress manufacturing inventories, X_3=wholesale inventories, and X_4=retail inventories; Xd_i, $i=1, \ldots, 4$ are the durable-goods portions of the X_i; and other variables are as previously

defined. Consider any category X_i which exhibits a persistent response to nominal-demand shocks. Then, for each such category, for all $j>0$:

[A] if the propagation process chiefly involves time-to-build, the b_{ij} in the regression for $L(Xd_i)$ are jointly significant, while for the $L(X_i-Xd_i)$ equation, $b_{ij}=0$; and, [B] if the propagation process chiefly involves Blinder-Fischer, the b_{ij} in both regressions are jointly significant.

Hypothesis Stemming from the Wage-Stickiness Propagation Mechanism

A final testable hypothesis is an implication of the wage-stickiness propagation mechanism advanced by Fischer (1977a); specifically, it is a formalized version of

Proposition 19: If the wage-stickiness propagation mechanism of Fischer is the fundamental explanation of the persistent response of real GNP to nominal-demand shocks, then the amount of persistence exhibited by the components of GNP should be in proportion to the amount of wage-stickiness exhibited within that component.

Implementing a test of Proposition 19 requires a measure of wage-stickiness (or, more accurately, of wage-stickiness relative to product-price-stickiness). Above (Chapter 2, pp. 66-67) it was argued that an appropriate measure of wage-stickiness is, for any category X_i,

$$d[L(P_i)] - d[L(W_i)],$$

where, $d[\cdot]$ is the first-difference operator, P_i is the average price of X_i and W_i is the average wage paid in the X_i sector. In sectors characterized by substantial wage-stickiness, a nominal-demand shock in t should cause the percent change in prices to exceed the percent change in wages for a number of periods subsequent to the shock. In contrast, in sectors where wage-stickiness is of minimal importance, the above expression should show little or no persistence. Thus a series of Barro-type regression equations of the form

$$d[L(P_i)]-d[L(W_i)] = A_i + c_{i0}DMR + c_{i1}DMR1 + c_{i2}DMR2 + \ldots + c_{ij}DMRj + \ldots + c_{in}DMRn,$$

$$\text{for } i=1, \ldots, k,$$

where P_i and W_i are prices and wages for X_i and other variables are as previously defined, ought to reveal a persistent impact of nominal-demand shocks in proportion to the amount of wage-stickiness in sector i. Further, if the wage-stickiness hypothesis is the key to explaining persistence in category i, then

$$L(X_i) = A_i + v\{d[L(P_i)]-d[L(W_i)]\},$$

where v is a parameter to be estimated and other variables are as previously defined, ought to be a specification with good explanatory power. (The estimated value for v should vary directly with the labor-intensity of the production processes in the X_i sector.) Substituting for $d[L(P_i)]-d[L(W_i)]$ in the above

expression yields a restriction on the behavior of $L(X_i)$ which should not be rejected if the wage-stickiness propagation mechanism is the key to persistence. That is, these considerations imply

Hypothesis 13: Given a series of Barro-type regression equations of the form

$$f(X_i) = A_i + b_{i0}DMR + b_{i1}DMR1 + b_{i2}DMR2 + \ldots + b_{ij}DMRj + \ldots + b_{in}DMRn, \text{ for } i=1, \ldots, k,$$

where $f(X_i)$ is either $(X_i/Y)\cdot L(Y)$ or $L(X_i)$, X_i is the ith component of real GNP, and other variables have the meanings previously assigned. Also given a corresponding series of regression equations of the form

$$d[L(P_i)]-d[L(W_i)] = A^*_i + c_{i0}DMR + c_{i1}DMR1 + c_{i2}DMR2 + \ldots + c_{ij}DMRj + \ldots + c_{in}DMRn, \quad \text{for } i=1, \ldots, k,$$

where P_i and W_i are prices and wages for X_i. Then a regression equation of the form

$$f(X_i) = A^{**}_i + v[c_{i0}DMR + c_{i1}DMR1 + c_{i2}DMR2 + \ldots + c_{ij}DMRj + \ldots + c_{in}DMRn],$$

where

$$A^{**}_i = A_i + vA^*_i,$$

and the c_{ij} are those previously estimated in the regression with $d[L(P_i)]-d[L(W_i)]$ as dependent variable, provides a fit which is not significantly worse than that provided by the unrestricted regression equation.

For those GNP-categories where the implication of the wage-stickiness mechanism contradicts that of the time-to-build mechanism--for example, in a category where there is little wage-stickiness but where average production periods are long--a test arises which directly compares the explanatory power of the two propagation mechanisms. Several such cases will be considered in Chapter 4.

CHAPTER 4
EMPIRICAL RESULTS

Introduction

In Chapter 3, a number of testable hypotheses were developed from the logic of the three propagation mechanisms under investigation in this study. This chapter reports the findings which result from carrying out tests of these hypotheses. All of the empirical work adopts one variant or another of Barro's reduced-form technique for measuring the impact of "unanticipated money growth" on real macroeconomic variables (reviewed in detail in Chapter 2, pp. 15-20). In stage one, a nominal-demand forecasting equation is specified along lines already developed in the literature, and a series of nominal-demand shocks is derived from the forecasting equation. In stage two, a vector of explanatory variables--composed of current and lagged measures of nominal-demand shocks as well as certain natural-rate variables--is fitted to a dependent variable of interest (either a function of some component of GNP or of some related series). The explanatory power of the lagged nominal-demand-shock terms is then assessed, and conclusions are drawn as to the consistency of results with one or another of the propagation mechanisms under consideration in this study.

The next section discusses preliminary issues. Then, the equations used to generate nominal-demand shocks are presented

and discussed. Following that, empirical results are presented in five groups; specifically, into those stemming from the following: the disaggregation of GNP alone, the disaggregation of the investment accounts within GNP in combination with independent survey data on production periods, the analysis of the persistence of "starts," the disaggregation of inventory stocks, and analysis stemming from the wage-stickiness propagation mechanism.

Preliminary Issues

Prior to presenting results, it is convenient to briefly discuss two issues of significance to the entire study (issues relating to only a part of the empirical work will be discussed at the point of relevance). Two such issues are treated here: the nature and source of the data used in the study, and the conventions adopted throughout regarding the specification of the second-stage equations.

The Data

The data employed in the study are almost exclusively U.S. government and Federal Reserve System data: Specific sources are given in Appendix A. The government sources are mainly the Departments of Commerce and Labor, with the bulk of the data coming from Commerce. The Federal Reserve System is the source of most of the monetary and financial data. In general, sample periods cover a good portion, though not always all, of the U.S. post-World War II era. Data previous to 1946 are used in estimating several of the nominal-demand-shock equations, chiefly

where there is a precedent set in the literature which it was thought desirable to follow.

Both annual and quarterly data are utilized in the study, but the samples end at different times, the annual sample ending in 1985, and the quarterly sample ending in 1979:III. The reason for truncating the quarterly data is discussed in detail in the next section (pp. 198-199, below), and has to do with the instability exhibited by the quarterly nominal-demand equations when the post-1979 era is included in the quarterly sample. Except where otherwise stated, all quarterly data are seasonally adjusted. Table 4-1 defines all variables which appear in the text or tables below.

Specification of Second-Stage Equations

Since a large number of second-stage equations are to be estimated in the present study, several simplifications regarding the specification of these equations are adopted and utilized throughout the study. First, a uniform adjustment for serial correlation is implemented across all annual and quarterly equations, a second-order adjustment being adopted for equations using annual data and an eighth-order adjustment being adopted for quarterly equations. This is done even though many of the equations show evidence of substantially less serial correlation than that which the models assume. Given that error processes are not to be assessed and adjusted for on an equation-by-equation basis, it seems more appropriate to overadjust rather than to underadjust for serial correlation across all equations. The consequence of overadjustment is mainly the loss of degrees

TABLE 4-1: DEFINITIONS OF ALL VARIABLES

Operators:

$L(\cdot)$ = $Ln(\cdot)$.

Xi = the value of X in period t-i.

Variables Appearing in Nominal-Demand Forecasting Equations:

DM = L(M)-L(M1) = where M is "M1": the rate of growth of the "M1" definition of the money stock.

FEDV = Barro's measure of real Federal Government spending relative to normal.

LUR = L(U/(1-U)), where U is the unemployment rate in the total labor force, including military personnel.

DP = L(P)-L(P1), where P is the GNP deflator (1982=100).

CP = Commercial Paper discount rate.

DNOMY = L(PY)-L[(P1)(Y1)]; the growth rate of nominal GNP.

DDEBT = L(DEBT)-L(DEBT1), where DEBT is the real value of the federal debt.

DB = L(B)-L(B1), where B is the monetary base.

FEDVP (annual data) = 0.8[FEDV1+DFGE], where DFGE is the predicted value from Equation 4-2-6.

FEDVP (quarterly data) = 0.2[FEDV1+DFGE], where DFGE is the predicted value from Equation 4-3-6.

WAR = dummy variable set to equal zero except for the year (quarter) after the end of a war (1946, 1954, 1973), when it equals the yearly (quarterly) average of the number who had served in the military for the preceding war. See Rush (1986, p. 263) for further details.

TB = Treasury Bill rate.

TABLE 4-1--continued

Definitions of Nominal-Demand Shock Concepts:

DMR = Barro-type measure of unanticipated money growth.

DMRGA = Gordon-type measure of unanticipated money growth for annual data, version "A."

DMRGB = Gordon-type measure of unanticipated money growth for annual data, version "B."

DMRG = Gordon-type measure of unanticipated money growth for quarterly data.

DMRM = Mishkin-type measure of unanticipated money growth.

DMRN = Barro-type measure of unanticipated money growth, derived using joint estimation of a real-GNP and Barro-type money-growth equation.

DBR = Rush-type measure of unanticipated monetary-base growth.

DYRG = Gordon-type measure of unanticipated nominal-GNP growth.

DYRM = Mishkin-type measure of unanticipated nominal-GNP growth.

Variables from Second-Stage Equations:

aj = jth-order autoregressive parameter.

t = a time trend.

LFG = L(FG), see below.

Y = real GNP.

Disaggregation of GNP

C = real consumption expenditures (CDUR+CPER+CSERV).

CDUR = real consumer expenditures on duroble consumer goods.

TABLE 4-1--continued

CPER = real consumer expenditures on nondurable (perishable) consumer goods.

CSERV = real consumer expenditures on services.

GPDI = real gross private domestic investment expenditures.

DEP = real depreciation expenditures.

NPDI = real net private domestic investment expenditures.

NRFI = real nonresidential fixed investment expenditures.

PDUR = real producers' durable expenditures.

STR = real investment in nonresidential structures.

H = real investment in residential structures.

DINVY = real change in business inventories.

(NRFI, PDUR, STR, H, and DINVY are net of depreciation expenditures for annual data, and gross--including depreciation expenditures--for quarterly data).

G = real government expenditures.

FG = real federal government expenditures.

SLG = real state and local government expenditures.

NETX = real U.S. net exports.

Disaggregation of producers' durable expenditures

PDINF = information processing and related equipment.

PDINFA = PDINF less "office, computing, and accounting machinery."

PDINDL = industrial equipment.

TABLE 4-1--continued

PDTRANS = transportation and related equipment.

PDTRBIG = that portion of PDTRANS composed of "aircraft, ships, boats, and railroad equipment."

PDTRSMALL = that portion of PDTRANS composed of, (a) "trucks, buses, and truck trailers," and, (b) "autos."

PDTRCKS = that portion of PDTRSMALL composed of "trucks, buses, and truck trailers."

PDAUTOS = that portion of PDTRSMALL composed of "autos."

PDELSE = other equipment (PDUR - PDINF - PDINDL - PDTRANS).

"Starts" concepts

ODUR = real value of manufacturers' new orders for durable goods (including defense-goods industries).

ONDK = real value of manufacturers' new orders for nondefense capital goods.

CAM = real value of newly approved capital appropriations, 1000 largest manufacturing corporations (ranked by total assets).

OCON = construction contracts awarded for commercial and industrial buildings, square feet of floor space.

HS = new private housing units started (annual rate, thousands of units).

OH = index of new private housing units authorized by local building permits.

"MORE" = manufacturers' evaluation of their plant and equipment facilities: percent saying "more needed."

"LESS" = manufacturers' evaluation of their plant and equipment facilities: percent saying "less needed."

PLANONE = planned expenditures for new plant and equipment by all industries, one quarter ahead, as a percentage of actual expenditures.

TABLE 4-1--continued

PLANTWO = planned expenditures for new plant and equipment by all industries, two quarters ahead, as a percentage of actual expenditures.

Disaggregation of real inventory stocks

INVYS = real business inventories.

FIG = real business finished-goods inventories.

FIGDU = durable-goods component of FIG.

FIGND = nondurable-goods component of FIG.

WIP = real business work-in-progress inventories.

WIPDU = durable-goods component of WIP.

WIPND = nondurable-goods component of WIP.

MAS = real business materials-and-supplies inventories.

WH = real business wholesale-trade inventories.

WHDU = durable-goods component of WH.

WHND = nondurable-goods component of WH.

RET = real business retail-trade inventories.

RETDU = durable-goods component of RET.

RETDUAUT = portion of RETDU held by outo dealers.

RETDU-AUT = RETDU - RETDUAUT.

RETND = nondurable-goods component of RET.

Variables appearing in wage-stickiness test equations

P-WCONN = d[L(P)]-d[L(W)], where P is the implicit price deflator for nonresidential construction, ond W is construction-worker averoge hourly earnings.

TABLE 4-1--continued

P-WCONR	=	d[L(P)]-d[L(W)], where P is the implicit price deflator for residential construction, and W is construction-worker average hourly earnings.
P-WDUR	=	d[L(P)]-d[L(W)], where P is the implicit price deflator for producers' durable goods, and W is durable-goods-industry worker average hourly earnings.
P-WNDUR	=	d[L(P)]-d[L(W)], where P is the implicit price deflator for consumer nondurable goods, and W is nondurable-goods-industry worker average hourly earnings.
P-WSERV	=	d[L(P)]-d[L(W)], where P is the implicit price deflator for consumer services, and W is service-industry worker average hourly earnings.

Note: See Appendix A for data sources.

of freedom, while the consequences of underadjustment are potentially severe (Granger and Newbold, 1974, has an extended discussion).

A second issue relates to the lag length N for the lagged nominal-demand shocks in the second-stage equations. A uniform lag length was chosen for all equations, a length of 3 years being chosen for annual data and 16 quarters being chosen for quarterly data (the discrepancy between the annual and the quarterly lag lengths stems from an unwillingness to sacrifice a degree of freedom in the annual data). The value chosen for N is somewhat larger than what would be suggested by Barro's work, but a generous lag length is desirable due to the work of Mishkin (1983), who claims that Barro's findings stem from improper truncation of the lag lengths of money-shock variables in output and unemployment equations (discussed in Chapter 2, above).

A third issue concerns the choice of a vector of natural-rate variables for the second-stage equations. Here two simplifications are adopted and uniformly applied across all equations. First, the same natural-rate vector is imposed across all equations (with one exception).[1] Second, only the natural-rate variables used most consistently by Barro--a time

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[1]In the federal government spending equations the log of federal government spending (LFG) is dropped as an explanatory variable. It can be argued that LFG should be dropped as an explanatory variable in the total government spending (G) equation as well. This adjustment was tried with little difference in results. Accordingly, the variable was retained in the total government expenditures regression equation.

trend[2] and the log of federal government spending (LFG)--are used as explanatory variables, so as to avoid questions about the extent to which results are due to experimentation with the contents of the natural-rate vector.[3]

Specification of the Nominal-Demand Forecasting Equations

Implementation of the Barro empirical procedure requires that a forecasting equation be specified from which measures of nominal-demand shocks can be derived. A number of methods for specifying such equations have been developed in the literature. However, the most influential and best-known procedure doubtless remains the original approach introduced by Barro (1977b, 1978, 1981b) and Barro and Rush (1980) in specifying their money-growth forecasting equation. The disproportionate influence of the Barro-Rush approach makes it desirable that results using this approach be emphasized in this study. Nonetheless, the use of only the Barro-Rush technique would open the present study up to the charge leveled against the research of Barro and his

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[2]While, strictly speaking, a time trend is not a natural-rate variable, its coefficient estimate does have statistical characteristics, and it will be referred to--perhaps somewhat inaccurately--as a natural-rate variable throughout this study.

[3]Previous experimentation did successfully isolate some variables which seemed to become important when the 1981-85 period was added to the sample; specifically, a lagged measure of the variance of interest rates, the real value of a fuel price index, and (though the case for its inclusion is debatable) a lagged ex post real interest rate variable. Also for the various investment equations a measure of the average corporate-profits tax rate was found to have good explanatory power. Including these variables mainly reduced the degree of serial correlation, sometimes substantially, but did not alter the overall results. Moreover, the impact of these variables typically was not robust to the first-differencing test for specification suggested by Plosser and Schwert (1978) and adopted by Barro in his research (Barro, 1981b).

followers; specifically, that Barro incorrectly specifies his forecasting equation. If such is the case, then Barro's measure of "unanticipated money growth" is inappropriate and his results can be discounted or even dismissed due to poor methods.

A number of criticisms along these lines (reviewed in detail in Chapter 2) exist in the literature. Gordon (1980, 1982) has argued that "unanticipated growth in nominal GNP" should be substituted for "unanticipated money growth" in the second-stage equations, on the grounds that use in this capacity of money shocks rather than nominal-GNP shocks amounts to the invalid assumption that the velocity of money follows a random walk over time. Gordon (1982) and Pesaran (1982) have criticized researchers such as Barro for including a contemporaneous explanatory variable in the forecasting equation (FEDV, a measure of federal expenditures relative to normal), on the grounds that including such a variable, which is unlikely to be known by forecasters until the next period, overstates the ability of forecasters to predict future money growth. Gordon (1982), following Nelson (1975) and McCallum (1979), also has maintained that failure to include the vector of second-stage natural-rate variables in a forecasting equation (as Barro fails to do) leads to inconsistent estimates of individuals' money-growth forecasts. Merrick (1983) has argued that the failure to include an interest-rate variable in a forecasting equation is a potentially serious omission from Barro-type money-growth equations. Mishkin (1983) has suggested that an atheoretical, "Granger-causality" type of specification process, used to select from a wide range

of candidate explanatory variables, should be adopted instead of Barro's procedure (where the researcher chooses from among a small number of potential explanatory variables primarily via reference to economic theory). Finally, Rush (1986) raises the possibility that there are circumstances (not those faced by Barro) in which use of the monetary base instead of "M1" is appropriate in the forecasting equation.

The empirical work reported below addresses all of these concerns by exploring the robustness of results to changes in nominal-demand-shock concept. With respect to money shocks, in addition to Barro's approach, versions both of Gordon's approach and of Mishkin's are utilized; further, a version of Rush's approach, which uses the monetary base rather than "M1" as the monetary variable, also is employed. With respect to nominal-GNP shocks, versions of both Gordon's and Mishkin's approach to specification are utilized. One or more of these nominal-demand-shock concepts is immune from each of the charges leveled above against the Barro approach to specifying a forecasting equation. Below, following a detailed presentation and discussion of results arrived at using the better-known Barro-type shock concepts, summaries of results derived using other shock concepts will be supplied which are sufficiently detailed so as to give an indication of the robustness of results to a change in shock concept.

Annual Nominal-Demand Forecasting Equations

This section presents the nominal-demand forecasting equations which are employed in those portions of the study

utilizing annual data. Table 4-2 presents seven nominal-demand forecasting equations--five money-growth equations and two nominal-GNP equations--as well as a federal-spending forecasting equation (used in generating Rush-type monetary-base shocks).[4] Equation 4-2-1 is a Barro-type "M1"-growth forecasting equation for the years 1943-85.[5] Generally speaking, the methods used in generating the equation corresponded to Barro's methods. The explanatory variables are Barro's. Further, as in Barro (1978, 1981b) and Barro and Rush (1980), all observations for the war years 1941-45 are multiplied by 0.36 (including the constant term).[6] By and large the equation performs well, suggesting that, at least for annual data, Barro's procedure is robust to an extension of the sample through 1985. Coefficients and t-statistics are similar to those reported in Barro (1981b) for the 1941-78 period. Two tests of residual randomness, Durbin's h-statistic and the Q-statistic, are employed and both fail to suggest a lack of randomness in the residuals. Finally, the sample was split between 1979 and 1980 and a Chow test was run to investigate the possibility that the process generating money

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[4]All estimation throughout the study was carried out using SAS (Statistical Analysis System). Some data transformations were carried out using Lotus 1-2-3.

[5]All sample periods reported in this study allow for the loss of early observations due to the necessity of generating lagged variables. Thus, the data used to estimate the Barro-type "M1"-growth forecasting equation begins in 1941, but after generating the explanatory variable DM2, the "effective sample" remaining starts in 1943.

[6]Implementing this adjustment involves redefining the constant term to equal 0.36 in the war years and one in other years. Thus the regression does not have a true constant term and, as is well known, R^2 tends to be biased upwards in such cases (it is reported nonetheless).

TABLE 4-2: NOMINAL-DEMAND FORECASTING EQUATIONS, ANNUAL U.S. DATA

4-2-1: Barro-Type Equation, 1943-85

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	0.091	4.77
DM1	0.424	3.19
DM2	0.217	1.71
FEDV	0.070	3.90
LUR1	0.029	4.50
R^2	0.925[a]	
s	0.016	
F	93.088**	
Durbin's h	-0.738	
Q (6 lags)	3.090	

4-2-2: Gordon-Type Equation, Natural-Rate Variables Included, 1944-85

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	-0.199	-2.30
DM1	0.287	2.71
DM2	0.249	2.66
DP1	-0.007	-0.07
CP1	-0.002	-1.23
t	0.001	2.51
LFG	0.027	2.19
R^2	0.817	
s	0.016	
F	26.015**	
Durbin's h	0.059	
Q (6 lags)	2.200	

TABLE 4-2--continued

4-2-3: Gordon-Type Equation, Natural-Rate Variables Excluded, 1944-85

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	0.010	1.54
DM1	0.427	4.07
DM2	0.201	1.98
DP1	-0.170	-2.04
CP1	0.003	3.31
R^2	0.752	
s	0.018	
F	28.087**	
Durbin's h	-0.684	
Q (6 lags)	2.840	

4-2-4: Mishkin-Type Equation, 1957-85

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	0.019	2.46
DM1	0.381	2.00
DM2	0.381	2.36
DNOMY1	-0.002	-1.31
DDEBT1	0.144	2.74
R^2	0.717	
s	0.014	
F	15.183**	
Durbin's h	0.088	
Q (6 lags)	1.470	

TABLE 4-2--continued

4-2-5: Rush-Type Equation, 1943-85

Dependent Variable = DB

Explanatory Variable	Coefficient	t-statistic
Constant	0.065	2.20
DB1	0.564	5.27
FEDVP	0.070	4.93
LUR1	0.018	1.79
R^2	0.702	
s	0.026	
F	30.561**	
Durbin's h	-0.636	
Q (6 lags)	6.090	

4-2-6: Rush-Type Federal-Spending Prediction Equation, 1943-85

Dependent Variable = DLFG

Explanatory Variable	Coefficient	t-statistic
Constant	0.243	1.42
DLFG1	0.381	4.83
LUR1	0.070	1.22
WAR	-0.413	-6.89
R^2	0.694	
s	0.154	
F	29.504**	
Durbin's h	-0.137	
Q (6 lags)	13.77*	

TABLE 4-2--continued

4-2-7: Gordon-Type Equation, 1948-85

Dependent Variable = DNOMY

Explanatory Variable	Coefficient	t-statistic
Constant	19.223	1.16
DM1	99.928	2.67
DM2	-76.753	-2.47
DNOMY1	-0.228	-1.21
DP1	27.964	0.91
TB1	-0.834	-2.36
t	0.321	2.84
LFG	-5.365	-1.74
R^2	0.489	
s	2.831	
F	4.095**	
Durbin's h	-0.251	
Q (6 lags)	1.890	

4-2-8: Mishkin-Type Equation, 1948-85

Dependent Variable = DNOMY

Explanatory Variable	Coefficient	t-statistic
Constant	7.233	5.35
DM1	126.829	3.54
DM2	-67.292	-1.95
DNOMY1	-0.352	-1.72
DP1	48.449	1.81
TB1	-0.292	-1.17
R^2	0.323	
s	3.154	
F	3.053*	
Durbin's h	0.557	
Q (6 lags)	2.030	

TABLE 4-2--continued

Notes: See Table 4-1 for definitions of variables. F-test is for the statistical significance of the entire regression equation.

*Significant at the five percent level.

**Significant at the one percent level.

$^{a}R^2$ is overstated. See the text's discussion of Equation 4-2-1 for details.

growth changed with the announced decision of the Federal Reserve in October 1979 to pay more attention to money growth and less attention to interest rates. No evidence suggesting the existence of such a structural shift was found. This last result accords with that of Evans (1984), who, also using annual data, found little change in coefficients for a Barro-type forecasting equation when the sample period was altered from 1947-78 to 1947-81.

Equation 4-2-2 is an adaptation to annual data of Gordon's (1982) specification of a quarterly money-growth forecasting equation. Gordon's quarterly specification includes "four lagged values of changes in nominal GNP, money, and the GNP deflator and two lagged values of the commercial paper rate (Gordon, 1982, p. 1099). For annual data, this was converted into two lagged values of actual money growth (DM), and a single lag each of the commercial paper rate and the change in the GNP deflator (Gordon's other variable, the change in nominal GNP, was dropped due to poor performance). Gordon has also asserted that all natural-rate variables in the second-stage equation ought also to be included in the forecasting equation (in order to assure that the measures of nominal-demand shocks are "orthogonal to the other predetermined variables in the second-stage equations . . ." (Gordon, 1982, p. 1096)). Accordingly, both LFG and t are included as explanatory variables. (It should be noted that including these variables conflicts in the present paper with other advice by Gordon (1982, p. 1097) that contemporaneous explanatory variables should not be included in the forecasting

equation.) Finally, inclusion by Gordon of the lagged commercial paper rate as an explanatory variable meets Merrick's (1983) criticism of Barro for failing to include a lagged interest-rate variable (as also do several other nominal-demand-shock equations used in this section).

Equation 4-2-3 is a second version of a Gordon-type money-growth forecasting equation, identical in specification to Equation 4-2-2 except for the omission of the two natural-rate variables LFG and t. Despite Gordon's advocacy of the inclusion of variables of this type in the forecasting equation, his advice can be questioned on several reasonable grounds. First, it can lead to a specification which directly contradicts another of Gordon's suggestions (as indicated above in the discussion of Equation 4-2-2). Second, theoretical grounds are lacking for the inclusion of these variables in the forecasting equation. The statistical significance of both LFG and t in Equation 4-2-2 adds weight to this consideration, as does the substantial difference between coefficient estimates on those explanatory variables which are present both in Equations 4-2-2 and 4-2-3. Finally, given the importance assigned to the issue by Gordon, McCallum (1979), and Nelson (1975), it is interesting, on general principles, to see how robust results are to inclusion of the vector of natural-rate variables in the forecasting equation. Comparison of results using residuals from Equation 4-2-2 with results stemming from use of residuals taken from Equation 4-2-3 allows this.

Equation 4-2-4 is an adaptation of Mishkin's atheoretical procedure of generating a forecasting equation, which involves the use of Granger-causality tests. In this procedure, the nominal-demand variable is regressed against one or more of its own lagged values plus an extensive list of other lagged macroeconomic variables. All lagged values of a variable are retained in the forecasting equation if they are jointly significant at the chosen significance level. Mishkin's method does not allow the researcher to "fine-tune" the specification process for the forecasting equation, meaning that it hinders the researcher from choosing a set of explanatory variables which would generate a forecasting equation confirming the researcher's prior expectations--a major advantage, in his view. Mishkin's procedure was adapted to annual data by using a single lag of each of the candidate explanatory variables, and then retaining only those which were significant at the five percent level. With one exception (the balance of payments on current account), Mishkin's candidate variables also were used in the process leading to Equation 4-2-4. Candidate variables were the unemployment rate,[7] the Treasury Bill rate, the High-Employment Budget Surplus, and changes in each of the following: "M1," the GNP deflator, nominal GNP, real federal-government spending, and the national debt. The final specification includes the High-Employment Budget Surplus, a variable for which

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[7]Barro's measure of the unemployment rate {LUR=L[U/(1-U)]} is used rather than U or L(U). Mishkin does not make clear exactly what function of the unemployment rate he uses in his study.

the author could find data only after 1954. Consequently, the effective sample for the Mishkin-type "M1" forecasting equation covers only the 1957-85 period.

Equation 4-2-5 is a version of Rush's monetary-base-growth forecasting equation, while Equation 4-2-6 is a version of Rush's federal-spending prediction equation (used in generating the variable FEDVP in Equation 4-2-5). Both of these equations and the motivations behind them are described in detail in Rush (1986). The main advantage of this equation is that use of shocks generated from it allows a check of robustness of results to a change in the concept of money from "M1" to the monetary base. In addition, Equation 4-2-5 avoids the use of a contemporaneous explanatory variable (since FEDVP= 0.8[FEDV1+DFGE], where DFGE is the predicted value from Equation 4-2-6).

Equations 4-2-7 and 4-2-8 are two nominal-GNP-growth forecasting equations, used at the suggestion of Gordon (1982) and Mishkin (1983). Gordon argues that a money-growth forecasting equation in essence assumes away without reason the problem of changes in the velocity of money. The strengths and weaknesses of Gordon's argument are discussed in detail above (Chapter 2, pp. 73-75), where it is concluded that, despite the theoretical merits of Gordon's view, an important weakness is that forecasting nominal-GNP-growth is much more difficult than forecasting money growth. On these grounds one might expect that attempts to specify a nominal-GNP-growth forecasting equation would be much less successful than attempts to specify money-

growth forecasting equations. Gordon does not shed any light on this issue, as (remarkably) he does not report any of his estimated forecasting equations, the grounds asserted being that "the estimated coefficients . . . play no role in our subsequent analysis" (Gordon, 1982, p. 1100). However, inspection of Equations 4-2-7 and 4-2-8 reveal that, at least in the present study, nominal-GNP-growth forecasting equations perform very poorly relative to money-growth forecasting equations. In both Equations 4-2-7 and 4-2-8, the decline in R^2s and the drop in the value of the F-statistic testing for the significance of the entire regression equation is precipitous. While both F-statistics remain significant, this is so only at the five percent level for Equation 4-2-8 (which differs from Equation 4-2-7 only in the exclusion of the two natural-rate variables, the inclusion of which cannot be justified on theoretical grounds). Moreover, both for Equations 4-2-7 and 4-2-8, the pattern of coefficients on the two lagged DMs is difficult to interpret. One might well question results derived for second-stage equations which use as explanatory variables the residuals of poorly-performing forecasting equations such as Equations 4-2-7 or 4-2-8. Whether Gordon's highly influential results are subject to such criticisms remains unknown.

Equation 4-2-7 is a Gordon-type nominal-GNP forecasting equation. Gordon utilized an identical specification for his money-growth and nominal-GNP-growth equations. This is also done here, with the exception that the Treasury-Bill rate (TB) and not the commercial-paper rate (CP) is the interest-rate variable in

Equation 4-2-7 (substituting TB for CP in 4-2-7 makes the specification of Equations 4-2-7 and 4-2-8 identical except for the inclusion of natural-rate variables in Equation 4-2-7).

Equation 4-2-8 is a Mishkin-type nominal-GNP-growth forecasting equation. In truth, associating Equation 4-2-8 with Mishkin is somewhat inaccurate, since Mishkin's atheoretical specification procedure broke down when an attempt was made to use it in this case. However, the explanatory variables retained in Equation 4-2-8 are taken from among those in Mishkin's set of "candidate variables." Further, unlike Equation 4-2-7, no natural-rate variables from second-stage equations are included in Equation 4-2-8. Since Equations 4-2-7 and 4-2-8 are identical except for the inclusion of the natural-rate variables in Equation 4-2-7, further information can be gained about the robustness of results to this inclusion.

All of the nominal-demand forecasting equations in Table 4-2 fail to show evidence of serial correlation in the residuals. Further, a Chow test splitting the sample between 1979 and 1980 failed to reveal evidence of instability in any of the forecasting equations.

Quarterly Nominal-Demand Forecasting Equations

Quarterly forecasting equations are specified in ways analogous to the annual equations. An important difference between them, however, is that the samples differ somewhat: The samples for the quarterly equations begin in 1949:II (1949:III for the Barro-type equation) and end in 1979:III. An initial attempt was made to specify quarterly equations over a sample

ending in 1985. However, when the sample was split between 1979:III and 1979:IV in order to test for any change in the money-supply process that may have occurred at this time, Chow tests rejected the hypothesis of no change in the coefficients for four of the six nominal-demand-shock equations utilized in the quarterly work. Only a Rush-type monetary-base equation and a Gordon-type nominal-GNP-growth equation failed to reject the null hypothesis. The implication is that, while on a year-to-year basis the announced Federal Reserve policy shift has not altered the essentials of the money-supply ("M1") process, on a quarter-to-quarter basis the change has been significant. Under these circumstances, it was decided to omit the post-1979:III sample from all of the quarterly work.[8]

Table 4-3 presents six nominal-demand forecasting equations, plus a federal-spending prediction equation used in generating the Rush-type monetary-base equation. Equation 4-3-1 is a Barro-type forecasting equation, modelled after that of Barro and Rush (1980). There are two main differences between Equation 4-3-1 and the Barro-Rush equation. First, Equation 4-3-1 has a sample period starting somewhat later than that of Barro-Rush, one consequence being that there is no need to carry out the special adjustment for World War II years of the sample that Barro and Rush carry out (and which is also done in the annual equation 4-2-1, above). Second, the equation has left out several

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[8]Some preliminary work was carried out using the shocks generated from the unstable forecasting equations in an attempt to explain unemployment, real output, and the components of real output. Surprisingly, results were similar to those for the truncated sample.

TABLE 4-3: NOMINAL-DEMAND FORECASTING EQUATIONS, QUARTERLY U.S. DATA

4-3-1: Barro-Type Equation, 1949:III-1979:III

Dependent Variable = DM

Explanatory Variable	Coef-ficient	t-stat-istic
Constant	0.016	3.46
DM1	0.773	8.59
DM2	-0.311	-2.73
DM3	0.281	2.45
DM4	-0.193	-1.69
DM5	0.239	2.68
FEDV	0.008	1.33
LUR1	0.005	3.11
R^2	0.605	
s	0.005	
F	24.684**	
Durbin's h	0.116	
Q (6 lags)	0.630	
Q (24 lags)	18.270	

TABLE 4-3--continued

4-3-2: Gordon-Type Equation, 1949:II-1979:III

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	0.008	0.84
DM1	0.647	6.42
DM2	-0.164	-1.35
DM3	0.214	1.84
DM4	-0.052	-0.49
DNOMY1	0.056	1.17
DNOMY2	0.002	0.04
DNOMY3	-0.023	-0.52
DNOMY4	0.013	0.28
DP1	-0.131	-1.33
DP2	-0.025	-0.25
DP3	0.073	0.82
DP4	-0.030	-0.35
CP1	-0.002	-3.34
CP2	0.002	2.89
t	0.0001	2.21
LFG	-0.002	-0.80
R^2	0.642	
s	0.005	
F	11.768**	
Durbin's h	0.167	
Q (6 lags)	4.060	
Q (24 lags)	25.180	

TABLE 4-3--continued

4-3-4:[a] Mishkin-Type Equation, 1949:II-1979:III

Dependent Variable = DM

Explanatory Variable	Coefficient	t-statistic
Constant	0.001	1.37
DM1	0.782	8.33
DM2	-0.170	-1.41
DM3	0.215	1.91
DM4	-0.067	-0.69
TB1	-0.004	-3.67
TB2	0.005	2.85
TB3	-0.001	-0.61
TB4	0.0004	0.39
R^2	0.624	
s	0.005	
F	23.460**	
Durbin's h	-0.578	
Q (6 lags)	4.660	
Q (24 lags)	35.810	

TABLE 4-3--continued

4-3-5: Rush-Type Equation, 1949:II-1979:III

Dependent Variable = DB

Explanatory Variable	Coef-ficient	t-stat-istic
Constant	0.014	2.33
DB1	0.239	2.58
DB2	0.085	0.95
DB3	0.216	2.49
DB4	0.274	3.23
LUR1	0.015	2.22
LUR2	-0.032	-2.36
LUR3	0.035	2.61
LUR4	-0.014	-2.07
FEDVP	0.033	1.01
R^2	0.641	
s	0.005	
F	22.202**	
Durbin's h	-0.216	
Q (6 lags)	1.450	
Q (24 lags)	21.530	

4-3-6: Rush-Type Federal-Spending Prediction Equation, 1948:IV-1979:III

Dependent Variable = DLFG

Explanatory Variable	Coef-ficient	t-stat-istic
Constant	0.011	0.36
DLFG1	0.517	5.61
DLFG2	0.098	1.10
LUR1	-0.091	-2.77
LUR2	0.094	2.88
WAR	-0.038	-0.92
R^2	0.488	
s	0.031	
F	22.487**	
Durbin's h	0.140	
Q (6 lags)	11.340	
Q (24 lags)	30.200	

TABLE 4-3--continued

4-3-7: Gordon-Type Equation,
1949:II-1979:III

Dependent Variable = DNOMY

Explanatory Variable	Coefficient	t-statistic
Constant	0.087	3.91
DM1	0.603	2.61
DM2	-0.077	-0.28
DM3	0.676	2.55
DM4	-0.102	-0.42
DNOMY1	0.098	0.88
DNOMY2	0.095	0.90
DNOMY3	-0.182	-1.77
DNOMY4	-0.155	-1.52
DP1	0.079	0.35
DP2	0.269	1.21
DP3	-0.079	-0.39
DP4	0.163	0.82
CP1	-0.002	-0.91
CP2	-0.0005	-0.25
t	0.0001	1.33
LFG	-0.019	-3.39
R^2	0.472	
s	0.010	
F	5.855**	
Durbin's h	-0.578	
Q (6 lags)	6.760	
Q (24 lags)	15.750	

TABLE 4-3--continued

4-3-8: Mishkin-Type Equation,
1949:II-1979:III

Dependent Variable = DNOMY

Explanatory Variable	Coefficient	t-statistic
Constant	0.055	4.41
DM1	0.605	2.55
DM2	0.047	0.16
DM3	0.534	1.97
DM4	-0.183	-0.76
LUR1	-0.020	-1.22
LUR2	0.036	1.27
LUR3	0.011	0.40
LUR4	-0.013	-0.89
TB1	-0.001	-0.20
TB2	-0.001	-0.14
TB3	0.002	0.45
TB4	-0.002	-0.64
R^2	0.413	
s	0.011	
F	6.397**	
Durbin-Watson	1.944	
Q (6 lags)	6.220	
Q (24 lags)	17.410	

Notes: See Table 4-1 for definitions of variables. F-test is for the statistical significance of the entire regression equation.

*Significant at the five percent level.

**Significant at the one percent level.

[a]No Equation 4-3-3 is defined in Table 4-3, in order to maintain the consistency of equation labels in Tables 4-2 and 4-3.

explanatory variables included in the Barro-Rush equation; specifically, one additional DM lag and two additional LUR lags. In both cases, poor variable explanatory performance explains the omissions. Despite these differences, the equation is similar to that of Barro and Rush: Most of the explanatory power of changes in "M1" is in the first DM lag, and the remaining coefficients on the DMs are somewhat difficult to interpret.

Equation 4-3-2 is a Gordon-type "M1"-growth forecasting equation. The vectors of DMs, DNOMYs, DPs, and CPs are identical to those in the forecasting equation used by Gordon (1982). Also, the two natural-rate variables from the second-stage equations, LFG and t, are included in the specification (the reason for this inclusion has been discussed in Chapter 2 (above, pp. 77-78)).

Equation 4-3-4[9] is a Mishkin-type money-growth equation developed according to the methods outlined in Mishkin (1983) (discussed above where the development of the annual equations is described). Vectors of one- through four-quarter lags of the same candidate variables as listed previously for the annual work were tried, and a set was retained only if it was jointly significant at the five percent level. Using this method eliminated all candidate vectors except those composed of "M1"-growth rates and Treasury-Bill rates, which led directly to Equation 4-3-4.

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[9]There is no Equation 4-3-3, so as to preserve conformity between equation designations in the annual and the quarterly work. Only a single Gordon-type money-growth equation is utilized in the quarterly work.

Equation 4-3-5 is an adaptation to quarterly data of the monetary-base prediction equation developed in Rush (1986), while Equation 4-3-6 is an adaptation of the annual federal-spending prediction equation developed in Rush (1986) (used to generate FEDVP). Rush's annual equation contained one annual lag each of DB and LUR, and a contemporaneous value of FEDVP (this variable is discussed further in Table 4-1, above). To convert to quarterly data, a four-quarter lag was substituted for each annual lag in Rush's specification. An analogous strategy was initially tried for Equation 4-3-6, but ultimately DLFG3 and DLFG4 were dropped from this specification due to the prediction equation's resulting poor performance.

Equation 4-3-7 is an adaptation of the nominal-GNP forecasting equation developed in Gordon (1982). The specification is Gordon's, with the exception that the vector of natural-rate variables utilized in this study is substituted for Gordon's vector (as militated by Gordon's method). The reservations about a nominal-GNP forecasting equation raised in the discussion of the Equations 4-2-7 and 4-2-8 apply to the quarterly versions as well. In particular, the marked decline in the explanatory power of Equation 4-3-7, as measured both by R^2 and the F-test for the significance of the entire regression, is noteworthy.

Equation 4-3-8 is a nominal-GNP forecasting equation derived following Mishkin's Granger-causality specification procedure. The procedure led to a specification where vectors of "M1" growth, unemployment rates, and Treasury-Bill rates are included

as explanatory variables. Again, the explanatory power is low in comparison with that of money-growth forecasting equations.

Neither Durbin's h-test (or, for Equation 4-3-8, the Durbin-Watson statistic) nor the Q-test for residual randomness indicate that the hypothesis of no serial correlation in the residuals can be rejected at traditional significance levels for any of the quarterly nominal-GNP forecasting equations.

Tables 4-4 and 4-5 present correlation coefficients between, respectively, the eight types of annual, and the seven types of quarterly, nominal-demand shocks utilized in the study.[10] Several aspects of the two tables deserve notice. First, residuals generated from the "M1"-growth equations are highly correlated, a result which confirms the earlier finding of Makin (1982), who observed high correlation among residuals generated from three other money-growth forecasting equations (all of which apparently used the same concept of money, though Makin does not explicitly state this). Second, Rush-type monetary-base shocks exhibit little correlation with either the other money shocks or nominal-GNP shocks. Finally, while the two measures of nominal-GNP shocks are highly correlated with each other, they are not highly correlated with any of the money-shock terms. Thus, considerable differences exist between "M1" shocks, monetary-base shocks, and nominal-GNP shocks. A second-stage equation which exhibits similar results across these various shock concepts can

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[10]The tables include one shock concept which has not yet been discussed; specifically, DMRN, which is derived from joint estimation of the real GNP and Barro-type DM equation. This shock concept will be discussed below along with joint estimation in general.

TABLE 4-4: PEARSON CORRELATION COEFFICIENTS BETWEEN ANNUAL NOMINAL-DEMAND SHOCKS

	DMR	DMRGA	DMRGB	DMRM	DMRN	DBR	DYRG	DYRM
DMR	1.000	0.865	0.847	0.911	0.971	0.273	0.495	0.508
DMRGA	0.865	1.000	0.862	0.825	0.869	0.219	0.538	0.574
DMRGB	0.847	0.862	1.000	0.827	0.843	0.295	0.492	0.614
DMRM	0.911	0.825	0.827	1.000	0.895	0.438	0.327	0.370
DMRN	0.971	0.869	0.843	0.895	1.000	0.312	0.457	0.505
DBR	0.273	0.219	0.295	0.438	0.312	1.000	0.347	0.346
DYRG	0.495	0.538	0.492	0.327	0.457	0.347	1.000	0.869
DYRM	0.508	0.574	0.614	0.370	0.505	0.346	0.869	1.000

Note: See Table 4-1 for definitions of variables.

TABLE 4-5: PEARSON CORRELATION COEFFICIENTS BETWEEN QUARTERLY NOMINAL-DEMAND SHOCKS

	DMR	DMRG	DMRM	DMRN	DBR	DYRG	DYRM
DMR	1.000	0.909	0.894	0.960	0.422	0.318	0.307
DMRG	0.909	1.000	0.937	0.859	0.398	0.349	0.332
DMRM	0.894	0.937	1.000	0.866	0.363	0.356	0.311
DMRN	0.960	0.859	0.866	1.000	0.470	0.297	0.267
DBR	0.422	0.398	0.363	0.470	1.000	0.108	0.071
DYRG	0.318	0.349	0.356	0.297	0.108	1.000	0.924
DYRM	0.307	0.332	0.311	0.267	0.071	0.924	1.000

Note: See Table 4-1 for definitions of variables.

be taken to show substantial robustness to changes in shock concept.

Results Stemming from the Disaggregation of Real GNP

This section reports results of carrying out tests of Hypotheses 1, 2, and 3 (developed above, Chapter 3, pp. 153-156). First, the persistence of unemployment and real GNP is established within the two-stage estimation framework originally developed and employed by Barro. Next, the joint estimation of money-growth and real GNP equations is presented and discussed. Then, GNP is disaggregated by type of product, the persistence of each component is investigated, and results are evaluated in the context of the above-mentioned hypotheses. Analysis is carried out and reported both for annual and for quarterly data, and the issue of robustness of results to variation in shock concept is explored in detail.

Annual Results Using Barro-Type Money Shocks

Annual results are presented in Tables 4-6 through 4-10. Tables 4-6 and 4-7 present results for unemployment, real GNP, and its components, using two-stage estimation and Barro-type money shocks as shock concept. Table 4-8 presents the results stemming from joint estimation of money-growth and real GNP equations, again using a Barro-type forecasting equation. Tables 4-9 and 4-10 report abbreviated results for unemployment, real GNP, and the components of real GNP, using shock concepts other than Barro-type two-stage-estimation shocks.

Results for unemployment and real output: two-stage estimation

Equations 4-6-1 and 4-6-2 of Table 4-6 are second-stage equations for unemployment and real GNP, using Barro-type money shocks within the context of a two-stage estimation procedure. The findings are similar to those reported by Barro in his studies of the postwar U.S. economy. Both a current and a single lagged DMR have a statistically significant and correctly-signed impact on unemployment and output. A second lagged DMR just misses statistical significance in the unemployment equation. The pattern of DMR coefficients is similar in both equations, revealing both persistence and momentum of unemployment and output in response to money shocks. Q-statistics fail to indicate the presence of serial correlation in the residuals. Finally, an F-test of the hypothesis that the three lagged money-shock terms are jointly insignificant is strongly rejected both for unemployment and output. Equations 4-6-1 and 4-6-2 thus establish that the persistent response of unemployment and real output to Barro-type money shocks is a result which is robust to an extension of the annual sample through 1985.

Joint estimation of money-growth and real output equations

Equation 4-2-1 (the Barro-type money-growth forecasting equation) and Equation 4-6-2 are estimated using a two-step, rather than a joint, estimation procedure. As previously discussed (above, Chapter 2, pp. 86-89), the two-step method generates estimates which are less efficient than the analogous estimates derivable by joint estimation. A further difficulty is that two-step methods yield inconsistent estimates of standard

errors of coefficient estimates, so that traditional hypothesis testing employing two-step procedures can lead to incorrect inferences. For these reasons, it is important to establish the robustness of results to a change from two-stage to joint estimation methods.

Table 4-8 presents the results of jointly estimating the Barro-type money-growth equation with the real output equation. Joint estimation was carried out by adapting the Nonlinear Least Squares program of Mishkin (1983) to the present estimation problem. An attractive feature of Mishkin's program is that its estimates converge to Maximum Likelihood Estimates (Mishkin, 1983, p. 18). Inspection of Table 4-8 reveals results for both the real-output and the money-growth equations which are similar to those derived using two-stage procedures.

Where it can be effectively implemented, the joint estimation method delivers gains over the two-step method. However, when attempting to use joint estimation to estimate the equations for the various components of GNP, serious difficulties are encountered which can only be partially overcome. The problem is that it is unclear how many of the disaggregated equations should be combined with the money-growth forecasting equation to form the group to be jointly estimated. Theoretically the best solution would be to jointly estimate the money-growth equation and all the GNP-component equations; however, high computational and programming costs make this approach impractical given the large number of equations that would need to be jointly estimated. On the other hand,

estimating a money-growth equation jointly with only a single GNP-component equation also would not be appropriate: If the advantage of the joint procedure is that it uses information from all equations in estimating each equation's coefficients, then to include along with the money-growth equation only a single GNP-component equation would affect the estimates of money-growth-equation coefficients in a way that at best is unclear.

Under these circumstances a compromise procedure is adopted which will be labelled "one-and-a-half-stage estimation." First, the money-growth and real-GNP equations are jointly estimated. Then, the residuals from the resulting money-growth equation are taken and used as measures of money shocks in explaining the behavior of the various components of real GNP. These residuals, which have been estimated using all information available in the system, should have properties that are superior to those derived using two-stage estimation methods. Results using the "one-and-a-half-stage" residuals as explanatory variables in second-stage equations will be reported below, where the issue of the robustness of overall results to changes in shock concept is discussed.

Results for the components of GNP: Hypothesis 1

The main idea underlying this section is that disaggregating real GNP and applying the Barro procedure to the disaggregated components ought to yield information on the nature of the mechanism which propagates the real effects of money shocks over time. The remainder of Table 4-6 is laid out along these lines.

TABLE 4-6: ANNUAL EQUATIONS FOR UNEMPLOYMENT, REAL GNP, AND THE COMPONENTS OF REAL GNP, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1946-85 U.S. DATA, "(X/Y)·L(Y)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF GNP)

	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory Variable[a]	LUR	L(Y)	C	NPDI	G	NETX
Constant	-2.14	5.18	4.51	0.89	-1.86	1.08
	(-3.6)	(41.1)	(19.4)	(2.0)	(-8.2)	(3.6)
t	0.03	0.03	0.04	-0.002	-0.01	-0.002
	(8.4)	(34.3)	(29.9)	(-1.1)	(-8.9)	(-1.1)
LFG	-0.46	0.09	-0.41	-0.04	0.81	-0.18
	(-3.6)	(3.4)	(-8.3)	(-0.4)	(17.0)	(-2.9)
DMR	-6.11	0.79	-0.74	3.27	-0.35	-0.65
	(-3.8)	(3.4)	(-1.3)	(2.8)	(-0.7)	(-1.1)
DMR1	-10.17	1.09	-0.93	3.67	-0.76	-0.41
	(-5.8)	(3.5)	(-1.4)	(2.9)	(-1.3)	(-0.5)
DMR2	-3.49	0.31	0.56	-1.17	0.60	0.23
	(-1.9)	(1.0)	(0.8)	(-0.9)	(1.0)	(0.3)
DMR3	-0.17	0.33	0.10	0.72	0.22	-0.84
	(-0.1)	(1.4)	(0.2)	(0.6)	(0.4)	(-1.4)
a1	0.30	0.92	0.50	0.41	0.43	0.85
	(1.7)	(5.2)	(2.8)	(2.3)	(2.4)	(5.3)
a2	-0.09	-0.23	-0.21	-0.25	0.09	-0.47
	(-0.5)	(-1.3)	(-1.2)	(-1.4)	(0.5)	(-2.9)
R^2	0.84	0.998	0.99	0.47	0.96	0.77
s	0.15	0.02	0.05	0.10	0.05	0.05
Q (6 lags)	1.51	0.99	5.56	1.74	3.95	2.72
F(n,d)[b]	11.40**	6.76**	1.80	5.03**	1.93	1.96

TABLE 4-6--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) (1954-85) STR	(11) H	(12) DINVY
Constant	0.48	0.67	-0.14	-0.42	0.52	-0.03
	(2.4)	(4.5)	(-1.4)	(-2.7)	(2.6)	(-0.2)
t	0.001	0.001	-0.001	-0.002	-0.002	-0.001
	(0.5)	(1.7)	(-1.0)	(-7.3)	(-1.6)	(-1.0)
LFG	-0.05	-0.12	0.05	0.13	-0.04	0.03
	(-1.1)	(-3.6)	(2.7)	(4.4)	(-0.8)	(0.7)
DMR	1.16	1.11	-0.001	0.20	1.24	1.00
	(2.5)	(3.2)	(-0.0003)	(1.0)	(2.9)	(1.8)
DMR1	2.20	1.99	0.20	0.65	0.19	1.43
	(3.9)	(4.8)	(0.8)	(3.1)	(0.4)	(2.5)
DMR2	1.07	1.03	0.06	0.35	-0.85	-1.58
	(1.8)	(2.4)	(0.2)	(1.7)	(-1.5)	(-2.7)
DMR3	0.33	0.27	0.08	-0.04	-0.05	0.43
	(0.7)	(0.7)	(0.4)	(-0.2)	(-0.1)	(0.7)
a1	0.61	0.61	0.82	0.37	0.71	0.03
	(3.6)	(3.6)	(5.0)	(2.0)	(4.3)	(0.1)
a2	-0.34	-0.32	-0.39	-0.47	-0.41	-0.16
	(-2.0)	(-1.9)	(-2.4)	(-2.5)	(-2.5)	(-0.9)
R^2	0.62	0.74	0.68	0.78	0.63	0.39
s	0.04	0.03	0.02	0.01	0.04	0.05
Q (6 lags)	4.18	7.35	4.60	6.20	2.82	0.67
F(n,d)[b]	5.17**	7.69**	0.36	3.47*	2.01	3.67**

TABLE 4-6--continued

Explanatory Variable[a]	(13) DEP	(14) CDUR	(15) CPER	(16) CSERV	(17) FG	(18) SLG
Constant	0.29	0.28	2.83	1.42	1.73	0.44
	(2.6)	(1.9)	(17.7)	(12.8)	(6.2)	(2.2)
t	0.01	0.01	-0.003	0.03	-0.01	0.01
	(10.0)	(13.9)	(-3.4)	(52.2)	(-3.0)	(6.7)
LFG	-0.04	-0.09	-0.11	-0.22		-0.07
	(-2.0)	(-2.8)	(-3.3)	(-9.2)		(-1.9)
DMR	-0.48	0.49	-0.65	-0.63	-1.43	-0.33
	(-2.6)	(1.7)	(-2.0)	(-2.0)	(-1.2)	(-0.9)
DMR1	-0.63	0.37	-0.87	-0.42	0.64	-0.65
	(-2.6)	(1.0)	(-2.1)	(-1.2)	(0.4)	(-1.4)
DMR2	-0.14	0.22	0.01	0.23	1.19	0.01
	(-0.6)	(0.6)	(0.04)	(0.6)	(0.7)	(0.03)
DMR3	-0.15	0.20	-0.20	0.06	0.13	0.03
	(-0.8)	(0.7)	(-0.6)	(0.2)	(0.1)	(0.1)
a1	0.97	0.81	0.68	0.35	1.16	0.77
	(5.5)	(4.9)	(3.9)	(2.1)	(7.6)	(4.3)
a2	-0.15	-0.37	-0.14	-0.30	-0.50	0.02
	(-0.8)	(-2.2)	(-0.8)	(-1.7)	(-3.3)	(0.1)
R^2	0.98	0.96	0.87	0.99	0.83	0.96
s	0.02	0.03	0.03	0.03	0.11	0.03
Q (6 lags)	4.59	2.45	6.19	6.25	2.35	15.66**
$F(n,d)^{b,c}$	3.83**	0.42	0.29	0.92	0.33	1.43

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness.

*Estimate is statistically significant at the five percent level.

TABLE 4-6--continued

**Estimate is statistically significant at the one percent level.

[a]Dependent variables in Columns (1) and (2) are as previously defined in Table 4-1. Others are the GNP-weighted shares of the ratio of the component to GNP. That is, they are of the general form, $(X/Y)\cdot L(Y)$, where X is some component of GNP.

[b]F-statistic is formed by restricting to zero the coefficients of all "relevant" lagged DMRs, where "relevant" is defined as specified on pp. 226-228, below. F(n,d)=F(3,31) except for the following: DINVY F(2,31), CPER F(2,31), FG F(3,32). Corresponding values with F(3,31) (that is, with all lagged DMR coefficients restricted to zero) for DINVY and CPER are given in Table 4-10.

[c]F-statistic is F(3,32) for the FG equation.

TABLE 4-7: ANNUAL EQUATIONS FOR UNEMPLOYMENT, REAL GNP, AND THE COMPONENTS OF REAL GNP, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1946-85 U.S. DATA, "L(X)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF GNP)

	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory Variable[a]	LUR[b]	L(Y)[b]	C	NPDI	G	NETX[c]
Constant			4.96	3.29	1.83	
			(52.5)	(2.8)	(15.1)	
t			0.03	0.02	0.02	
			(48.3)	(3.2)	(17.2)	
LFG			-0.01	0.08	0.56	
			(-0.4)	(0.3)	(24.5)	
DMR			0.42	7.97	0.18	
			(2.5)	(2.7)	(0.8)	
DMR1			0.54	8.19	0.21	
			(2.4)	(2.4)	(0.8)	
DMR2			0.19	-1.80	0.36	
			(0.8)	(-0.5)	(1.3)	
DMR3			0.17	1.72	0.26	
			(1.0)	(0.6)	(1.1)	
a1			0.93	0.43	0.82	
			(5.3)	(2.5)	(4.5)	
a2			-0.22	-0.25	-0.01	
			(-1.2)	(-1.4)	(-0.07)	
R^2			0.999	0.61	0.998	
s			0.01	0.27	0.02	
Q (6 lags)			2.48	1.04	23.63**	
F(3,31)[d]			2.82	3.43*	0.65	

TABLE 4-7--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) (1954-85) STR	(11) H	(12) DINVY[c]
Constant	2.96	4.91	-1.16	-2.44	2.90	
	(3.1)	(3.0)	(-1.3)	(-1.6)	(2.4)	
t	0.03	0.03	0.02	0.007	0.02	
	(5.9)	(4.0)	(4.7)	(2.2)	(2.5)	
LFG	-0.11	-0.69	0.60	1.02	0.03	
	(-0.5)	(-1.9)	(3.2)	(3.6)	(0.1)	
DMR	4.72	9.64	0.06	1.96	8.33	
	(2.1)	(2.5)	(0.04)	(1.0)	(3.2)	
DMR1	10.42	19.49	2.57	7.13	2.37	
	(4.0)	(4.3)	(1.1)	(3.4)	(0.7)	
DMR2	4.75	10.60	0.50	3.97	-2.79	
	(1.8)	(2.3)	(0.2)	(1.9)	(-0.8)	
DMR3	2.05	3.93	1.52	0.39	-0.45	
	(0.9)	(1.0)	(0.8)	(0.2)	(-0.2)	
a1	0.61	0.58	0.77	0.34	0.68	
	(3.7)	(3.3)	(4.6)	(1.8)	(4.1)	
a2	-0.36	-0.25	-0.38	-0.43	-0.36	
	(-2.1)	(-1.4)	(-2.3)	(-2.3)	(-2.2)	
R^2	0.82	0.74	0.88	0.80	0.63	
s	0.20	0.34	0.16	0.14	0.24	
Q (6 lags)	3.25	5.68	3.67	5.14	2.03	
F(3,31)[d]	5.54**	6.26**	0.97	4.61*	1.07	

TABLE 4-7--continued

Explanatory Variable[a]	(13) DEP	(14) CDUR	(15) CPER	(16) CSERV	(17) FG	(18) SLG
Constant	2.64	2.31	4.56	3.85	4.55	2.83
	(50.4)	(7.2)	(43.8)	(57.9)	(11.6)	(10.0)
t	0.04	0.05	0.02	0.04	0.01	0.04
	(75.6)	(27.9)	(31.0)	(62.0)	(2.3)	(17.1)
LFG	0.004	-0.09	0.03	-0.01		-0.04
	(0.4)	(-1.3)	(1.3)	(-1.1)		(-0.8)
DMR	-0.001	1.89	0.33	0.27	-1.49	0.20
	(-0.02)	(2.8)	(1.7)	(2.5)	(-1.0)	(0.4)
DMR1	0.07	1.90	0.43	0.42	1.17	-0.04
	(0.7)	(2.2)	(1.7)	(2.9)	(0.5)	(-0.1)
DMR2	0.09	0.72	0.19	0.15	1.00	0.17
	(0.8)	(0.8)	(0.8)	(1.0)	(0.5)	(0.3)
DMR3	0.04	0.66	0.12	0.11	0.13	0.09
	(0.4)	(1.0)	(0.6)	(1.0)	(0.1)	(0.2)
a1	1.11	0.74	0.85	0.99	1.08	0.79
	(6.4)	(4.4)	(4.8)	(5.6)	(6.6)	(4.4)
a2	-0.25	-0.34	-0.13	-0.16	-0.37	-0.001
	(-1.4)	(-2.0)	(-0.8)	(-0.89)	(-2.3)	(-0.005)
R^2	0.9998	0.99	0.997	0.9997	0.82	0.99
s	0.01	0.06	0.02	0.01	0.13	0.05
Q (6 lags)	17.43**	1.83	3.58	43.32**	3.71	21.92**
$F(3,31)^{d,e}$	0.24	2.15	1.16	4.17*	0.14	0.06

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness.

*Estimate is statistically significant at the five percent level.

TABLE 4-7--continued

**Estimate is statistically significant at the one percent level.

[a]Dependent variables of all estimated equations are of the form L(X), where X is some component of GNP.

[b]See Table 4-6 for estimated equation.

[c]This GNP-component contains negative values, so that its log is not defined for all of its values. Therefore, no equation has been estimated for this component.

[d]F-statistic is formed by restricting to zero the coefficients of all logged DMRs.

[e]F(3,32) for FG equation.

TABLE 4-8: JOINTLY ESTIMATED MONEY-GROWTH AND REAL OUTPUT EQUATIONS, ANNUAL DATA, 1948-85

	Dependent Variable = L(Y)	
Explanatory Variable	coefficient	asymptotic t-statistic
Constant	5.42	24.1
t	0.03	16.1
LFG	0.05	1.2
DMR	0.84	3.5
DMR1	1.20	3.7
DMR2	0.45	1.4
DMR3	0.31	1.3
a1	0.95	5.7
a2	-0.18	-1.0

	Dependent Variable = DM	
Explanatory Variable	coefficient	asymptotic t-statistic
Constant	0.07	1.8
DM1	0.30	1.9
DM2	0.25	1.7
FEDV	0.11	3.5
LUR1	0.03	3.6

Notes: See Table 4-1 for definitions of variables. ai is the

TABLE 4-8--continued

ith autocorrelation coefficient. Parameter estimates are derived using a version of Mishkin's (1983) nonlinear-least squares joint estimation program. See the text for details of estimation.

Equations 4-6-3 through 4-6-6 and 4-6-13, and the corresponding Equations 4-7-3 through 4-7-6 and 4-7-13, apply Barro's two-step estimation procedure to the analysis of the five main components of GNP: consumption expenditures (C), net private domestic investment (NPDI), government expenditures (G), net exports (NETX), and depreciation expenditures (DEP). The GNP-component equations in Table 4-6 measure the extent of persistence exhibited by the GNP-weighted ratios of a component X to GNP, while the equations in Table 4-7 measure the corresponding response of the (natural) log of some component X.

Equations 4-6-3 and 4-7-3 are the consumption equations. There is no evidence that Barro-type money shocks cause consumption expenditures to rise relative to output: Neither the current nor any of the lagged money-shock coefficients is significant in Equation 4-6-3, and the F-test for the joint significance of all three lagged shocks fails to reject the null hypothesis of no joint significance. In contrast, Equation 4-7-3 reveals some degree of persistence exhibited by the log of consumption: Not only DMR but also DMR1 has a statistically significant impact on L(C). However, the F-test fails to reject the null hypothesis of no significance of all three lagged shocks. It is therefore concluded that no substantial persistence of any kind is exhibited by C in response to Barro-type money shocks, at least in the annual data.

Equations 4-6-4 and 4-7-4 are the equations for net private domestic investment. Both the GNP-weighted ratio of NPDI to GNP, as well as the log level of NPDI, exhibit a persistent response

to Barro-type money shocks: The effect, however, is confined to DMR1 (in addition, the contemporaneous shock term is significant in both equations). F-tests for the joint significance of the three lagged DMRs are significant at the one percent level for both equations. NPDI thus exhibits the response to Barro-type money shocks which previously was labeled (Chapter 3, pp. 151-152, above) "strong persistence." In comparison to the other equations, R^2 in the NPDI equation is somewhat low, but this would appear to be due to the failure of either of the natural-rate variables to be statistically significant.

Equations 4-6-5 and 4-7-5 are the government-expenditures equations. No evidence from these equations suggests that any persistence is exhibited by G in response to Barro-type money shocks. One may raise the question of whether LFG is an appropriate explanatory variable in these equations; however, estimation of analogous equations without this explanatory variable also failed to find evidence of persistence. Moreover, the later equations for federal and state-and-local government spending confirm the no-persistence findings of Equations 4-6-5 and 4-7-5. It is therefore concluded that G does not exhibit a persistent response in the annual data to Barro-type money shocks.

Equation 4-6-6 is the net exports equation (since L(NETX) is undefined for negative values of NETX, there is no Equation 4-7-6). Again there is a general absence of evidence that this category responds to Barro-type shocks with persistence. Finally, Equations 4-6-13 and 4-7-13 are the equations for

depreciation expenditures. While the GNP-weighted ratio of DEP to Y shows strong negotive persistence--indicating that shocks couse the ratio to fall for severol periods subsequent to the date of the shock--the log-level DEP equation fails to show evidence of persistence. These results suggest that depreciation's fall relative to output is associated with the positive and persistent response of Y to shocks, rather than any response of DEP. One would expect such a result on time-to-build premises, since, as time-to-build generates a rise in net investment relative to total investment, a fall in depreciation expenditures relative to total output is to be expected.

It is now appropriate to examine Hypothesis 1 in the context of the above results. Hypothesis 1 stotes that a prediction of the time-to-build propagation mechanism is that "strong persistence" be confined to the investment accounts. Results for annual dota using Barro-type money shocks are consistent with the hypothesis. Only net private domestic investment responds with "strong persistence" to Barro-type money shocks. In fact, no other component of GNP unambiguously shows even a "weakly persistent" (and positive) response to such shocks. The implication is that an essential part of the process leading to the propagation of shocks over time is associated with the (net) investment accounts.

The meaning of the F-tests reported in Table 4-6 and in a number of subsequent tables in this section requires clarification. For most cases the coefficients of all three lagged shocks are restricted to zero, in which case

interpretation is straightforward. However, two F-tests in Table 4-6 (Equations 4-6-12 and 4-6-15) and others in subsequent tables leave one or more lagged shock coefficients unrestricted, a strategy requiring additional discussion. The idea is that, since the objective of the present study is to explain why lagged shocks have a positive and significant impact on real GNP, the important question to ask about the behavior of the components of GNP (or, later, about other variables of interest) is which of these also exhibits a positive and significant response to lagged money shocks. In this context, consider the problem of imposing appropriate restrictions on the consumer perishable expenditures equation (Equation 4-6-15), in which the coefficient on DMR1 is both negative and significant. A test for the joint significance of the three lagged shocks in this equation likely would reject the null hypothesis, but this is uninformative given that the objective is to determine whether this category responds positively to lagged shocks. Accordingly, in forming the restricted version of the model, DMR1 was retained in the specification, and only the coefficients on DMR2 and DMR3 were restricted to zero. A similar strategy was adopted for all other categories except depreciation expenditures and net exports.[11] However, while analysis focuses on the F-tests just described, Table 4-10 presents, for the sake of completeness, the analogous

[11] For the case of depreciation expenditures (and also LUR), the appropriate test would leave positive and significant coefficients on lagged shocks unrestricted (in fact, the issue did not arise in the annual work). For net exports, the implications of time-to-build are ambiguous (p. 99, above) and all lagged shocks therefore were restricted regardless of sign.

F-statistics where all DMRs are restricted to equal zero, in cases of the type just discussed.

Results for the components of GNP: Hypothesis 2

The evidence just discussed is consistent with Hypothesis 1 and the time-to-build propagation mechanism. It further strongly implies that the propagation process is mainly associated with the investment accounts. While these results are favorable to time-to-build, they are far from conclusive (for example, the results in principle could be explained by the Blinder-Fischer inventories-based mechanism as well). Additional relevant information concerning the propagation process can be obtained via disaggregation of NPDI into nonresidential structures (STR), residential structures (H), producers' durable expenditures (PDUR), and changes in inventories (DINVY); in addition, nonresidential fixed investment (NRFI=PDUR+STR) is included. The GNP-weighted shares of these variables form the dependent variables in Equations 4-6-7 through 4-6-12, while their corresponding log levels form the dependent variables in Equations 4-7-7 through 4-7-12 (however, since L(DINVY) is not defined for negative values of DINVY, no such equation can be estimated).

Equations 4-6-7 and 4-7-7 present the results for nonresidential fixed investment (NRFI). The findings are similar for the GNP-weighted ratio of NRFI to GNP and for L(NRFI): In both cases, a current and a single lagged DMR is statistically significant, and a second lagged DMR narrowly misses statistical

significance. In both equations, the F-test strongly rejects the null hypothesis that the lagged shocks are jointly insignificant.

Equations 4-6-8 and 4-7-8 are the producers'-durable expenditures-equations (PDUR). Again substantial persistence and momentum is observed in both equations; in fact, the most persistence of any category is observed for PDUR, as not only DMR1 but also DMR2 are statistically significant at the five percent level. F-tests for joint significance of all three lagged DMRs are significant at the one percent level.

Equations 4-6-9 and 4-7-9, and Equations 4-6-10 and 4-7-10, are two pairs of nonresidential-structures (STR) equations, estimated, respectively, over the standard 1946-85 period, and over the 1954-85 period. Over the 1946-85 period, neither dependent-variable concept exhibits persistence, a result which contradicts the predictions of time-to-build. However, Equations 4-7-9 and 4-7-10 reestimate the nonresidential structures equations over the 1954-85 period, with the result that "strong persistence" is exhibited by STR over this truncated sample. For both equations, the coefficient on DMR1 is positive and statistically significant, and the F-test for the joint significance of all lagged money shocks is significant at the five percent level. A further point of interest is that, if only 1953 is added to the 1954-85 sample, the equation's performance is about as poor as when the entire 1946-53 period is included in the sample. This last finding suggests that, in searching for an explanation for the anomaly in results between the two pairs of equations (as well as the anomaly between results for STR and the

other components of net investment), a focus on 1953-54 is appropriate.

Two important tax-related changes took place in the first half of the 1950s, either of which might plausibly account for what appears to be a pronounced structural change in the nonresidential-structures equation occurring around 1953-54. First, a major tax bill was passed in 1954 which allowed, for the first time, the general use of accelerated depreciation of capital-goods by businesses (as opposed to the previous practice--discussed below--of granting special exemptions to certain projects on a case-by-case basis). Such a fundamental change in the tax structure would be expected to have a particularly pronounced impact on nonresidential structures (the longest-lived of all capital goods). Other aspects of the 1954 tax act relating to depreciation also were favorable to investors in nonresidential structures (Hellmuth, 1955, has a detailed discussion). The net effect of these changes in the tax code may have been to fundamentally alter that relationship between nominal-demand shocks and nonresidential structures which prevailed over the 1946-53 period.

While the above might reasonably be expected to partly explain the failure of the nonresidential-structures equation to show persistence over the 1946-85 sample, an even more plausible explanation relates to special tax provisions passed in response to the Korean War "emergency" (as it was called at the time). In his discussion of the 1954 tax-reform law, Hellmuth points out that

> the new depreciation section makes available for all new depreciable assets the same type incentive offered for selected assets under the accelerated amortization programs of World War II and the Korean War. Accelerated amortization allowed full depreciation in five years, compared with the average depreciable life of about seventeen years for equipment and fifty years for plant, which apply under the (various accelerated-depreciation) methods. . . . Accelerated amortization is more favoroble to long-lived assets as a five-year writeoff is a greater gain on an asset with a fifty-year useful life than with a ten-year useful life. (Hellmuth, 1955, p. 337)

Additional discussion of the accelerated-amortization program of the Korean War years can be found in Machinery and Allied Products Institute (1952). The program granted accelerated-amortization privileges to those projects judged as being vital to defense purposes (some awards were granted for only a portion of project value, where the fraction of project value eligible was determined by the amount certified as ottributable to defense purposes). The size of the program was substantial: By May 1952 the value of the cumulated applications submitted by the private sector was over $30 billion, and certificates had by that date already been issued for about $20 billion (Machinery and Allied Products Institute, 1952, p. 40). By comparison, purchases of private nonresidential structures equalled $12.2 billion in 1952 (in 1953 it was $13.6, and by 1956 it was $18.2 billion). Thus the scope of the program was large enough to have had a substantial impact on investment in the Korean War period. Moreover, the program can account for two of those anomalies in results which previously have been noted; first, the fact that the failure of the 1946-85 specification is confined to the nonresidential-structures equation (where the impact of the

program would be largest), and, second, the fact that the Korean War year 1953 appears to be of special importance. Finally, inspection of the National Income and Product Accounts reveals that, over the 1953-54 period, real private nonresidential structures rose substantially (from $135.5 to $144.1 billion (1982=100)), while the real value of producers' durable expenditures fell substantially (from $78.3 to $73.2 billion (1982=100)). Such a pattern is consistent with the hypothesis that accelerated-amortization generated a major structural shift in the nonresidential-structures equation in the 1953-54 period.[12] In sum, while the failure of the nonresidential-structures equation to exhibit persistence over the full 1946-85 sample is a contradiction of the time-to-build hypothesis, the discrepancy can probably be accounted for by the considerable tax-law changes which characterized the era of the early 1950s.

In sharp contrast to the nonresidential-structures equations, the annual equations for residential investment (H)--Equations 4-6-11 and 4-7-11--are thoroughly consistent with the time-to-build hypothesis. No lagged shocks are significant in either equation, while the coefficient on the contemporaneous shock term DMR is both significant and positive. The F-test for

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[12]Another possible explanation hinges on monetary events during this period; specifically, the so-called "Accord" between the U.S. Treasury and the Federal Reserve System of March 1951, whereby the Reserve System would no longer support prices of Treasury bonds. Such an alteration in the relationship between monetary policy and interest rates might be expected to have a substantial impact on nonresidential construction, which is highly sensitive to changes in interest rates (for example, Bischoff, 1970, p. 10 expresses support for this view). However, the Accord, occurring in early 1951, cannot explain why the year 1953 appears to have special significance.

joint significance of all lagged shocks fails to reject the null hypothesis of no persistence for this category, indicating that lagged money shocks do not exert a statistically significant influence on residential investment.[13]

Having reviewed the annual results using Borro-type money shocks for the disaggregated net investment equations, the extent to which results are consistent with Hypothesis 2 can be examined. Hypotheses 2[A] and 2[B] together imply that "strong persistence" should be observed for the two categories STR and PDUR, while, for annual data, H should not exhibit either "strong" or "weak" persistence. As has been seen above, on balance the results are consistent with these propositions. Both PDUR and H respond as implied; in addition, the behavior of nonresidential fixed investment (NRFI) also is consistent with the predictions of time-to-build. The chief question mark is the behavior of the nonresidential-structures equation. Over the 1946-85 period, the performance of STR is blatantly contradictory with what would be predicted by time-to-build. Moreover, the result for STR conflicts drastically with the result for NRFI. However, truncating the sample to 1954-85 leads to results for STR which are broadly consistent with the time-to-build mechanism. To the extent that one is willing to believe that special problems exist when the 1946-53 period is included in the sample--special problems which are peculiar to the STR equation and do not even affect the NRFI equation--one can interpret the

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[13]The changes-in-inventories equation (Equation 4-6-12) will be discussed below in the context of Hypothesis 4.

results as being consistent with time-to-build. However, the anomalous behavior of STR cannot be dismissed as irrelevant (particularly in light of the quarterly results for this category presented below).

Hypothesis 2[C] states that, given what is known about average production periods in STR, PDUR, and H, STR should show the most persistence, followed by PDUR and then H. For the case of this restriction, results are mixed. The poor performance of STR over the 1946-85 sample already has been discussed. Comparing the STR equation for 1954-85 with the PDUR and H equations for 1946-85 reveals that, as expected, H exhibits the least persistence of the three categories. However, the maximum significant DMR lag M equals two for PDUR, while M equals only one for STR, so that, contrary to the predictions of time-to-build, producers' durable expenditures rather than nonresidential structures shows the most persistence. Comparison of the two pairs of equations also reveals that the persistence exhibited by PDUR is substantially stronger than that of STR over the 1954-85 sample. Thus the length and strength of the persistence in the producer-durables equation is greater than what would be expected on the basis of likely production periods for this category. Producers' durable expenditures thus responds with more persistence and momentum than any other investment category, a result which may well represent too much persistence to be fully explainable by reference to likely time-to-build effects. In sum, the results stemming from the test of Hypothesis 2, carried out for annual data using Barro-type money shocks, are mixed.

Results for the components of GNP: Hypothesis 3

It remains to discuss the disaggregation of consumption expenditures and total government expenditures. Equations 4-6-14 through 4-6-16, and 4-7-14 through 4-7-16, are the equations for consumers' durable-goods expenditures (CDUR), consumers' perishable-goods expenditures (CPER), and consumers' expenditures on services (CSERV). None of the three consumption components displays "strong persistence," although two--durable goods and services--display "weak persistence," while the consumer perishables equation narrowly misses displaying "weak persistence." The negative and significant coefficients on various DMRs and DMR1s in the GNP-weighted shares of CPER and CSERV suggest that shocks bring about a decline in the ratio of these categories to GNP. However, the significance or near-significance of DMR and DMR1 in the L(CPER) and L(CSERV) equations suggest that the decline in the component-to-GNP ratio is due not to an absolute decline in CPER and CSERV but rather to GNP's rising by more than the component (in response to a positive shock). The consumer-durable equations reveal a different pattern for this category, as the L(CDUR) equation exhibits persistence while the ratio of CDUR to Y does not respond to money shocks. This latter finding can be interpreted as evidence mildly favoring time-to-build, as one expects average production periods to be longer in CDUR then in CPER or CSERV, and CDUR shows the most persistent response of any consumption component. Finally, F-tests testing for the joint significance

of all lagged DMRs except those which are both "wrongly signed"[14] and statistically significant fail to reject the null hypothesis of no significance for all equations except that for L(CSERV). Thus CDUR and CPER display no (positive) persistence, while CSERV exhibits "weak persistence."

Equations 4-6-17, 4-6-18, 4-7-17 and 4-7-18 are the equations for federal-government expenditures and state-and-local government expenditures. No evidence of persistence is to be found in any of these equations.

It is now appropriate to examine Hypothesis 3, which asserts that, if time-to-build is the key to explaining persistence, then no subcotegory of consumption or government expenditures ought to display "strong persistence." Results are completely consistent with the hypothesis. Some "weak persistence" is exhibited by consumers' expenditures on services, but no other subcategory responds to Barro-type money shocks with persistence in the annual data.

Annual Results Using Alternative Measures of Nominal-Demand Shocks

The results presented obove use Barro-type money-growth shocks as measures of unanticipated changes in nominal demand. It remains to determine the robustness of the results reported above to changes in shock concept. Due to the large numbers of separate equations estimated, it seems most appropriate to report these supplementary findings in summary form only. Tables 4-9

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[14]The precise meaning of "wrongly signed" is discussed above, pp. 226-228.

and 4-10 present F-tests for the same second-stage-equation dependent variables as in Tables 4-6 and 4-7, using the eight shock concepts developed from Tables 4-2 and 4-8 above. The F-tests in Table 4-9 are like those reported in Tables 4-6 and 4-7, where the tests restrict to zero all lagged shock terms except those which are both statistically significant and signed so as to make their restriction inappropriate (the discussion on pp. 226-228, above, has details). Table 4-10 reports F-tests where all lagged shock terms are restricted to zero regardless of sign, for all cases in Table 4-9 where one or more lagged shocks is left unrestricted in forming the F-test. A number of important results emerge from an inspection of Table 4-9.

As a start, the robustness of the results for unemployment and real output are explored. As the first two columns of Table 4-9 indicate, both unemployment and real output continue to exhibit a persistent response to all four series of "M1" shocks. However, there is no evidence that either unemployment or real output responds with persistence to measures of nominal-GNP shocks. The critical nature of the controversy between those who focus solely on money shocks and those who advocate use of nominal-GNP shocks is highlighted by these results.[15] Monetary-base shocks are a mixed case, with unemployment responding with strong persistence, but with real output failing to respond with persistence. In sum, evidence that unemployment

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[15]However, quarterly results estimated over the 1954:I-1979:III period indicate that persistence in both real output and unemployment continues when nominal-GNP shocks are substituted for money shocks. This is discussed below.

TABLE 4-9: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, ANNUAL DATA, VARIOUS PERIODS FROM 1946-85, EIGHT SHOCK CONCEPTS

Shock Concept	LUR	L(Y)	C	
DMR	11.40**	6.77**	1.80	2.82
DMRGA	4.50*	3.33*	1.57	2.48
DMRGB	11.48**	9.11**	(1.48)	5.44**
DMRM	4.94*	3.54*	0.72	1.88
DMRN	7.87**	4.48*	1.24	3.00*
DBR	8.78**	1.33	0.04	1.32
DYRG	0.80	0.34	1.57	0.14
DYRM	2.00	1.21	(1.27)	0.13

TABLE 4-9--continued

Shock Concept	NPDI		G		NETX
DMR	5.03**	3.43*	1.93	0.65	1.96
DMRGA	1.75	1.04	(0.09)	0.24	0.66
DMRGB	6.32**	4.82**	(0.12)	0.69	1.72
DMRM	1.31	0.95	2.71	2.18	0.70
DMRN	3.06*	1.86	3.00*	0.97	1.34
DBR	0.92	1.08	0.21	0.64	0.10
DYRG	1.62	1.13	1.23	1.04	1.73
DYRM	(0.37)	1.50	3.17*	2.79	3.80*

TABLE 4-9--continued

Shock Concept	NRFI		PDUR		STR[b]	
DMR	5.17**	5.54**	7.70**	6.26**	0.36	0.97
DMRGA	1.27	1.40	3.05*	2.41	0.79	0.86
DMRGB	4.63**	4.92**	8.16**	6.83**	0.54	1.30
DMRM	4.22*	4.07*	2.31	2.48	7.26**	9.28**
DMRN	2.96	3.42*	4.36*	4.18*	0.27	0.42
DBR	4.10*	5.39**	8.89**	10.64**	0.41	0.41
DYRG	1.49	1.38	1.75	0.90	(0.78)	(0.46)
DYRM	2.51	2.47	3.62*	2.02	(0.23)	2.01

TABLE 4-9--continued

Shock Concept	STR (1954-85)		H		DINVY
DMR	3.47*	4.61**	2.01	1.07	(3.67)*
DMRGA	0.45	0.33	1.14	0.49	(2.06)
DMRGB	2.29	3.58*	3.10*	2.11	(4.73)*
DMRM[c]	7.26**	9.28**	0.50	0.17	0.82
DMRN	4.47*	5.49**	1.86	0.55	3.72*
DBR	1.63	2.14	0.29	0.06	0.05
DYRG	3.89*	3.90*	1.06	0.73	(0.21)
DYRM	2.74	3.34*	1.50	0.51	(0.80)

TABLE 4-9--continued

Shock Concept	DEP[a]		CDUR		CPER	
DMR	3.83*	0.24	0.42	2.15	(0.29)	1.16
DMRGA	1.78	0.02	1.56	1.12	(0.71)	1.23
DMRGB	6.53**	0.005	0.81	3.02*	(1.90)	3.30*
DMRM	2.07	0.25	(1.70)	1.61	0.57	1.67
DMRN	2.37	0.70	0.64	2.46	(0.92)	1.93
DBR	0.42	0.22	0.16	0.80	0.04	0.73
DYRG	0.26	2.43	0.32	0.66	(0.81)	0.29
DYRM	0.46	0.91	0.56	0.52	(2.20)	0.10

TABLE 4-9--continued

Shock Concept	CSERV		FG		SLG	
DMR	0.93	4.17*	0.33	0.14	1.43	0.06
DMRGA	0.52	2.40	2.11	2.12	(1.54)	(0.05)
DMRGB	(0.83)	4.43*	0.89	1.26	(0.69)	0.41
DMRM	0.78	3.24*	0.74	0.30	2.38	2.42
DMRN	0.95	2.10	1.48	1.04	2.00	0.24
DBR	0.34	0.49	0.72	0.76	0.03	0.07
DYRG	0.89	0.59	8.90**	5.84**	0.23	0.12
DYRM	1.40	1.02	3.98*	2.90	0.60	0.13

Notes: See Table 4-1 for definitions of variables. For LUR and L(Y), F-statistics are formed from regressions where the forms of

TABLE 4-9--continued

dependent variables are as defined in Table 4-1. For remaining F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated. For NETX and DINVY, some values of $L(X)$ are undefined, so that only left-hand-side Fs appear for these GNP-components.

Where all lagged shocks are restricted to zero, F-statistics have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data are available for all lags, and all samples end in 1985): DMR, F(3,31) {1946}; DMRGA and DMRGB, F(3,30) {1947}; DMRM, F(3,17) {1960}; DMRN, F(3,26) {1951}; DBR, F(3,31) {1946}; DYRG and DYRM, F(3,26) {1951}. Denominator degrees of freedom are one larger for the FG equations than for other equations, since the FG equations omit LFG as an explanatory variable.

For the definition of "relevant" in the present context, see the discussion on pp. 226-228, above. F-statistics in parentheses leave one or more lagged shocks unrestricted; others restrict all lagged shocks to zero. Following is a list of: (a) F-statistic degrees of freedom, and, (b) lagged shocks left unrestricted, for all Fs in parentheses (presented in the general form $F(n,d)\ \{\cdot\}$, where within brackets are the numbers corresponding to the DMR lags left unrestricted). First, for left-hand-side Fs: for DMR: DINVY F(2,31),{2}; CPER F(2,31),{1}. For DMRGA: G F(2,30),{1}; DINV F(2,30),{1}; CPER F(1,30),{1,3}; SLG F(1,30),{1,2}. For DMRGB: C F(1,30),{1,3}; G F(2,30),{1}; DINV F(2,30),{2}; CPER F(1,30),{1,3}; CSERV F(2,30),{1}; SLG F(2,30),{1}. For DMRM: CDUR F(2,17),{3}. For DMRN: CPER F(2,26),{1}. For DBR: none. For DYRG: STR F(2,26),{2}; DINVY F(2,26),{2}. For DYRM: NPDI F(2,26),{2}; C F(2,26),{1}; STR F(2,26),{2}; DINVY F(2,26),{2}; CPER F(1,26),{1,3}. Second, for right-hand-side Fs: for DMRGA: SLG F(2,30),{1}. For DYRG: STR F(2,26),{2}. For DMR, DMRGB, DMRM, DMRN, DBR, DYRM: none. F-statistics corresponding to the above which restrict all three lagged shocks to zero are presented in Table 4-10.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]For the case of DEP, equations with statistically significant F-statistics contain lagged shocks which are both negative and statistically significant, but no positive and significant lagged shocks. Note 11 of the text has a discussion.

[b]Recall that DMRM equation is estimated over the 1960-85 sample.

[c]1960-85 for STR equation where DMRM is the shock concept.

TABLE 4-10: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, ANNUAL DATA, VARIOUS PERIODS FROM 1946-85, EIGHT SHOCK CONCEPTS

Shock Concept	LUR	L(Y)	C
DMR			
DMRGA			
DMRGB			3.97*
DMRM			
DMRN			
DBR			
DYRG			
DYRM			3.25*

TABLE 4-10--continued

Shock Concept	NPDI	G	NETX
DMR			
DMRGA		3.30*	
DMRGB		4.13*	
DMRM			
DMRN			
DBR			
DYRG			
DYRM	2.66		

TABLE 4-10--continued

Shock Concept	NRFI	PDUR	STR	
DMR				
DMRGA				
DMRGB				
DMRM				
DMRN				
DBR				
DYRG			2.16	2.08
DYRM			1.94	

TABLE 4-10--continued

Shock Concept	STR (1954-85)	H	DINVY
DMR			5.51**
DMRGA			3.21*
DMRGB			6.13**
DMRM[n]			
DMRN			
DBR			
DYRG			3.30*
DYRM			5.17**

TABLE 4-10--continued

Shock Concept	DEP	CDUR	CPER
DMR			2.89
DMRGA			4.04*
DMRGB			5.62**
DMRM		2.09	
DMRN			2.26
DBR			
DYRG			4.86**
DYRM			6.92**

TABLE 4-10--continued

Shock Concept	CSERV	FG	SLG	
DMR				
DMRGA			3.99*	1.37
DMRGB	2.82		4.14*	
DMRM				
DMRN				
DBR				
DYRG				
DYRM				

Notes: See Table 4-1 for definitions of variables. For all F-statistics, the left-hand-side Fs are those from a regression

TABLE 4-10--continued

where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated. For DINVY, some values of $L(X)$ are undefined, so that only left-hand-side Fs appear for these GNP-components.

All Fs restrict the three lagged shocks to zero (omitted Fs are reported in Table 4-9). F-statistics therefore have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data is available for all lags, and all samples end in 1985): DMR, F(3,31) (1946); DMRGA and DMRGB, F(3,30) (1947); DMRM, F(3,17) (1960); DMRN, F(3,26) (1951); DBR, F(3,31) (1946); DYRG and DYRM, F(3,26) (1951).

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

and real output respond to money shocks with persistence is highly robust across a number of money-shock concepts, but does not extend to nominal-GNP shocks (at least in the annual data).

Turning to Hypothesis 1 and the first-order disaggregation of real output into C, NPDI, G, NETX, and DEP, results on the whole are supportive of the hypothesis. Of the five money-shock concepts which generate a persistent response of both unemployment and output, three of these also generate some form of "strong persistence" in NPDI. By contrast, among these same money-shock concepts there is no evidence of "strong persistence" in these shocks' impact on consumption (although there is intermittent evidence of a "weakly persistent" impact), and there is only minimal evidence of persistence for G. No persistence for NETX is observed for these shock concepts, and only negative persistence is found for DEP. Thus results using alternate shock concepts support previous findings by suggesting that, whatever the specific nature of the propagation mechanism, it mainly operates through the investment accounts. However, the findings for NPDI are fairly weak in comparison to the strong results stemming from the further disaggregation of NPDI, and thus the extent to which they are consistent with later results is unclear.

Hypothesis 2 involves investigating the persistence of those disaggregated components of NPDI other than changes in inventories (for which findings are discussed in a later section). Results are in the main consistent with the hypothesis, confirming the findings reported previously using

Barro-type shocks. Beginning with nonresidential fixed investment (NRFI=PDUR+STR), three out of the five shock concepts which generated "strong persistence" in LUR and L(Y) also generate "strong persistence" in NRFI. In addition, "strong persistence" is generated by DBR. These results are consistent with the time-to-build hypothesis, which holds that persistence ought to be concentrated in the fixed investment accounts.

Disaggregation of NRFI yields even stronger results for producers' durable expenditures (PDUR), where four shock concepts generate a response of "strong persistence" from this category, and where additional evidence of persistence is observed for two other shock concepts. Nonresidential structures (STR) over the 1946-85 period continue to fail to respond with persistence to nominal-demand shocks of any type. However, when (for the reasons discussed on pp. 229-232, above) the sample for this category is restricted to the post-Korean war era, results again are consistent with time-to-build. In this latter case, three of six money-shock concepts as well as one nominal-GNP-shock concept generate "strong persistence." Again, the interpretation of findings for STR depends on how comfortable one is with accepting the equation for the truncated sample period as representative of the basic behavior of STR. Findings also are similar to those derived using Barro-type shocks in that results for PDUR are somewhat stronger, and results for STR somewhat weaker, than what is predicted by the hypothesis that the time-to-build propagation mechanism is the only mechanism at work in the data. Finally,

residential construction (H) continues to respond in a manner consistent with Hypothesis 2, as only one of seven nominal-demand- shock concepts generates any kind of persistent response of this category.

Turning now to Hypothesis 3 and the results stemming from the disaggregation of C into CDUR, CPER, and CSERV, the very strong result emerging is the absence of evidence that any of the eight varieties of nominal-demand shocks generates "strong persistence." Some evidence of "weak persistence" exists, particularly for CSERV. Further, disaggregation of total government expenditures into federal expenditures (FG) and state-and-local expenditures (SLG) reveals no evidence that any of the six varieties of money shocks generates any type of a persistent response. However, nominal-GNP shocks do generate some evidence of "strong persistence" in federal spending.

Overall, the results stemming from use of alternative nominal-demand shocks lend further support to the proposition that the real effects of past nominal-demand shocks are confined to the investment accounts. The chief questions continue to concern the behavior of nonresidential structures, and what possibly is an overly-exuberant response of producers' durable expenditures to nominal-demand shocks.

Quarterly Results Using Barro-Type Money Shocks

Quarterly results derived using Barro's DMRs are somewhat more difficult to interpret than the corresponding annual findings. On balance, however, they support the previous conclusions. Data are utilized for the 1948:I-1979:III period

for all shock concepts except for Rush-type DBRs, for which the data begin in 1948:III (the latter due to the necessity of estimating the federal-spending prediction equation). However, 16 quarterly lags of shocks are utilized as explanatory variables in all second-stage equations, so that the first period for which data are available is 1953:III (1954:I for equations using DBRs). Because of the problems previously discussed with the Korean war era and nonresidential structures, and in order to establish a uniform sample period across all shock concepts, the two observations in 1953 were dropped, and all equations are estimated over the 1954:I-1979:III period. Two natural-rate variables, t and LFG, also are included as explanatory variables in all equations, and an adjustment for eighth-order serial correlation is carried out for the reasons previously discussed (p. 176, above). Findings are presented in Tables 4-11 through 4-15.

Results for unemployment and real output

Equations 4-11-1 and 4-11-2 are quarterly results for LUR and L(Y) derived using two-stage estimation procedures and Barro-type money shocks. Results indicate that substantial persistence and momentum characterize the relationship between DMRs, unemployment, and real output in the quarterly data. For both unemployment and output, Barro-type shocks have an immediate impact, which peaks at about four quarters and continues for about nine quarters. The F-statistics testing for the joint significance of all lagged DMRs for L(Y), and all "relevant"

shocks for LUR,[16] are significant at the one percent level for both equations. Other properties of the equations are satisfactory. Results are consistent not only with the findings of Barro and Rush (1980) but also with the annual results reported in the previous section.

Table 4-13 presents jointly-estimated quarterly equations for real output and money growth, using a Barro-type money-growth forecasting equation specification (identical to that in Equation 4-3-1 above) and a real-GNP equation with the same specification as for Equation 4-11-2. Estimation procedures, employing an adaptation of Mishkin's nonlinear least squares program, were along the lines previously specified when discussing the joint estimation of the annual output and money-growth equations. As was the case for the annual results, findings are similar to those arrived at using two-stage estimation procedures. The residuals from the money-growth equation in Table 4-13, called DMRN, are later used as explanatory variables in the quarterly version of the "one-and-a-half-stage" procedure which has been previously discussed for the annual work. Summaries of quarterly results using DMRN are presented in Tables 4-14 and 4-15, below.

Results for the components of GNP: Hypothesis 1

The remainder of Table 4-11 presents results where Barro-type shocks and two-stage estimation are used, where dependent

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[16]This is discussed further above (pp. 226-228). For all F-statistics in Table 4-11 where one or more lagged DMRs are not restricted, there remains the question of how much explanatory power the "wrongly-signed" DMRs have for the specification. This information is contained in the first row of Table 4-15, which reports the F-statistics where all lagged DMRs, whether positive or negative, are restricted.

TABLE 4-11: QUARTERLY EQUATIONS FOR UNEMPLOYMENT, REAL GNP, AND THE COMPONENTS OF REAL GNP, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1954:I-1979:III U.S. DATA, "(X/Y)·L(Y)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF GNP)

Explanatory Variable[a]	(1) LUR	(2) L(Y)	(3) C	(4) GPDI	(5) G	(6) NETX
Constant	-1.26	5.33	4.54	1.97	-1.24	-0.09
	(-1.6)	(37.6)	(12.2)	(4.5)	(-5.8)	(-0.3)
t	0.01	0.01	0.01	0.002	-0.002	-0.0002
	(7.9)	(66.8)	(27.7)	(7.0)	(-9.9)	(-0.7)
LFG	-0.50	0.08	-0.34	-0.26	0.70	0.02
	(-2.6)	(2.2)	(-3.8)	(-2.5)	(13.5)	(0.2)
DMR	-4.01	0.65	-0.77	2.47	-0.55	-0.46
	(-2.9)	(3.1)	(-1.3)	(2.7)	(-1.8)	(-1.1)
DMR1	-6.95	1.19	-0.80	4.51	-1.37	-1.08
	(-3.1)	(3.8)	(-1.0)	(3.6)	(-3.0)	(-1.8)
DMR2	-12.40	1.65	-1.09	5.99	-1.65	-1.62
	(-4.6)	(4.5)	(-1.2)	(4.3)	(-3.0)	(-2.4)
DMR3	-17.03	2.11	-0.94	7.52	-1.78	-2.77
	(-5.8)	(5.3)	(-1.0)	(5.1)	(-3.0)	(-3.7)
DMR4	-19.67	2.14	-0.68	7.02	-1.76	-2.76
	(-6.7)	(5.2)	(-0.7)	(4.7)	(-2.8)	(-3.5)
DMR5	-18.61	1.93	0.03	4.91	-1.46	-2.08
	(-6.4)	(4.6)	(0.03)	(3.3)	(-2.3)	(-2.5)
DMR6	-16.32	1.58	0.64	3.06	-0.91	-1.84
	(-5.9)	(4.0)	(0.7)	(2.2)	(-1.5)	(-2.3)
DMR7	-13.38	1.37	0.74	2.31	-0.54	-1.92
	(-5.0)	(3.5)	(0.8)	(1.7)	(-0.9)	(-2.5)
DMR8	-9.36	1.19	0.02	1.68	-0.36	-1.03
	(-3.6)	(3.1)	(0.02)	(1.3)	(-0.6)	(-1.3)
DMR9	-5.98	1.06	0.45	0.62	-0.16	-0.68
	(-2.3)	(2.8)	(0.5)	(0.5)	(-0.3)	(-0.9)
DMR10	-2.60	0.56	1.22	-1.16	0.80	-0.94
	(-1.0)	(1.6)	(1.4)	(-1.0)	(1.4)	(-1.3)
DMR11	0.94	0.32	0.70	-2.30	1.10	0.26
	(0.4)	(0.9)	(0.8)	(-1.9)	(2.0)	(0.4)
DMR12	3.53	0.33	0.54	-2.53	1.09	0.73
	(1.4)	(1.0)	(0.6)	(-2.1)	(2.0)	(1.1)
DMR13	5.12	-0.16	0.44	-2.25	1.23	0.18
	(2.1)	(-0.5)	(0.5)	(-1.8)	(2.4)	(0.3)
DMR14	4.66	-0.13	-0.28	-0.95	1.00	0.01
	(2.0)	(-0.4)	(-0.3)	(-0.8)	(2.1)	(0.01)
DMR15	4.28	-0.18	0.08	-0.82	0.72	-0.24
	(2.1)	(-0.6)	(0.1)	(-0.7)	(1.6)	(-0.4)
DMR16	2.15	-0.17	0.25	-0.45	0.24	-0.12
	(1.6)	(-0.9)	(0.4)	(-0.5)	(0.8)	(-0.3)

TABLE 4-11--continued

Explanatory Variable[a]	(1) LUR	(2) L(Y)	(3) C	(4) GPDI	(5) G	(6) NETX
a1	1.19	1.01	0.80	0.76	1.03	0.85
	(10.3)	(8.7)	(6.9)	(6.7)	(9.0)	(7.4)
a2	-0.34					
	(-1.9)					
a3						
a4						
a5						
a6		0.26				
		(1.6)				
a7		-0.24				
		(-1.5)				
a8				-0.19		
				(-1.7)		
R^2	0.96	0.999	0.99	0.91	0.99	0.89
s	0.05	0.01	0.03	0.04	0.01	0.02
Q (6 lags)	4.95	1.36	2.01	0.73	5.67	3.24
Q (24 lags)	18.67	14.52	16.94	15.71	12.81	15.36
F(n,d)[b]	4.27**	2.49**	0.60	2.21*	1.30	1.93*

TABLE 4-11--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) H	(11) DINVY
Constant	0.94	0.78	0.06	0.79	-0.05
	(4.7)	(4.4)	(0.8)	(3.8)	(-0.2)
t	0.002	0.002	-0.0003	0.0002	0.0002
	(8.9)	(10.9)	(-6.2)	(0.9)	(1.6)
LFG	-0.10	-0.13	0.06	-0.11	0.02
	(-2.1)	(-3.2)	(3.1)	(-2.3)	(0.4)
DMR	0.22	0.17	0.08	0.73	0.98
	(0.8)	(0.8)	(0.6)	(2.6)	(1.2)
DMR1	0.62	0.53	0.13	1.26	1.97
	(1.5)	(1.6)	(0.6)	(3.0)	(2.1)
DMR2	1.55	1.09	0.51	1.78	2.12
	(3.1)	(2.7)	(2.1)	(3.5)	(2.2)
DMR3	2.41	1.85	0.76	1.76	2.62
	(4.3)	(4.2)	(2.9)	(3.1)	(2.7)
DMR4	2.79	2.18	0.81	1.00	2.50
	(4.7)	(4.6)	(3.1)	(1.7)	(2.6)
DMR5	2.87	2.22	0.92	0.05	1.44
	(4.6)	(4.5)	(3.5)	(0.1)	(1.5)
DMR6	2.70	2.08	0.93	-0.29	0.24
	(4.5)	(4.3)	(3.8)	(-0.5)	(0.3)
DMR7	2.81	2.36	0.72	-0.51	-0.26
	(4.7)	(4.8)	(3.1)	(-0.9)	(-0.3)
DMR8	2.74	2.14	0.80	-0.41	-0.64
	(4.7)	(4.4)	(3.5)	(-0.7)	(-0.7)
DMR9	2.29	1.77	0.62	-0.98	-0.77
	(4.1)	(3.8)	(2.9)	(-1.8)	(-0.9)
DMR10	1.61	1.38	0.33	-1.17	-1.64
	(3.0)	(3.1)	(1.6)	(-2.2)	(-1.9)
DMR11	0.97	0.97	0.06	-1.18	-1.91
	(1.9)	(2.3)	(0.3)	(-2.3)	(-2.2)
DMR12	0.78	0.99	-0.21	-1.13	-1.62
	(1.5)	(2.4)	(-1.0)	(-2.3)	(-1.9)
DMR13	0.45	0.63	-0.21	-0.67	-1.49
	(0.9)	(1.6)	(-1.0)	(-1.4)	(-1.8)
DMR14	0.13	0.35	-0.32	-0.64	-0.05
	(0.3)	(1.0)	(-1.6)	(-1.4)	(-0.1)
DMR15	-0.26	0.12	-0.52	-0.33	-0.05
	(-0.7)	(0.4)	(-2.7)	(-0.8)	(-0.1)
DMR16	-0.56	-0.41	-0.28	0.03	0.09
	(-2.1)	(-1.9)	(-1.9)	(0.1)	(0.1)

TABLE 4-11--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) H	(11) DINVY
a1	1.11	1.02	0.87	1.08	0.37
	(9.6)	(8.9)	(7.6)	(9.3)	(3.2)
a2					
a3					
a4					
a5				0.26	
				(1.5)	
a6	-0.28	-0.23			
	(-1.7)	(-1.5)			
a7					
a8					-0.19
					(-1.6)
R^2	0.98	0.99	0.92	0.93	0.52
s	0.01	0.01	0.01	0.01	0.03
Q (6 lags)	8.02	7.92	1.80	1.64	0.20
Q (24 lags)	22.72	17.83	16.77	18.77	13.76
F(n,d)[b]	2.45**	2.52**	1.67	2.42*	1.13

TABLE 4-11--continued

Explanatory Variable[a]	(12) CDUR	(13) CPER	(14) CSERV	(15) FG	(16) SLG
Constant	0.95	1.96	1.65	1.44	0.38
	(4.6)	(8.4)	(9.0)	(36.7)	(2.1)
t	0.002	-0.0004	0.006	-0.01	0.003
	(13.1)	(-1.7)	(44.5)	(-14.0)	(14.0)
LFG	-0.17	-0.07	-0.10		0.03
	(-3.5)	(-1.3)	(-2.3)		(0.7)
DMR	0.25	-0.40	-0.54	0.42	-0.10
	(0.8)	(-1.2)	(-1.7)	(1.0)	(-0.4)
DMR1	0.79	-0.48	-1.20	0.71	-0.45
	(1.9)	(-1.0)	(-2.7)	(1.2)	(-1.3)
DMR2	0.99	-0.69	-1.61	0.54	-0.37
	(2.1)	(-1.3)	(-3.2)	(0.7)	(-0.9)
DMR3	1.30	-0.63	-1.52	0.83	-0.37
	(2.6)	(-1.1)	(-2.9)	(1.0)	(-0.8)
DMR4	1.49	-0.51	-1.72	1.25	-0.39
	(2.8)	(-0.8)	(-3.2)	(1.5)	(-0.8)
DMR5	1.39	-0.35	-1.23	1.93	-0.38
	(2.6)	(-0.6)	(-2.3)	(2.2)	(-0.8)
DMR6	1.22	0.07	-0.72	1.99	-0.24
	(2.3)	(0.1)	(-1.4)	(2.3)	(-0.5)
DMR7	0.97	0.07	-0.39	2.16	-0.08
	(1.8)	(0.1)	(-0.8)	(2.4)	(-0.2)
DMR8	0.66	-0.2	-0.55	2.44	-0.10
	(1.2)	(-0.3)	(-1.1)	(2.8)	(-0.2)
DMR9	0.90	-0.15	-0.37	2.44	-0.13
	(1.8)	(-0.3)	(-0.8)	(2.9)	(-0.3)
DMR10	0.37	0.57	0.26	2.26	0.41
	(0.8)	(1.0)	(0.6)	(2.27)	(0.9)
DMR11	-0.13	0.39	0.43	1.73	0.55
	(-0.3)	(0.7)	(1.0)	(2.2)	(1.3)
DMR12	0.22	-0.16	0.43	1.58	0.64
	(0.5)	(-0.3)	(1.0)	(2.1)	(1.5)
DMR13	-0.08	-0.17	0.71	1.33	0.63
	(-0.2)	(-0.3)	(1.6)	(1.8)	(1.5)
DMR14	-0.19	-0.32	0.33	1.16	0.56
	(-0.5)	(-0.6)	(0.7)	(1.7)	(1.5)
DMR15	-0.14	-0.22	0.50	1.39	0.35
	(-0.4)	(-0.5)	(1.2)	(2.3)	(1.0)
DMR16	-0.20	-0.003	0.49	0.35	0.07
	(-0.7)	(-0.01)	(1.6)	(0.8)	(0.3)

TABLE 4-11--continued

Explanatory Variable[a]	(12) CDUR	(13) CPER	(14) CSERV	(15) FG	(16) SLG
a1	0.83	0.88	0.87	1.01	0.96
	(7.2)	(7.6)	(7.5)	(8.8)	(8.3)
a2					
a3					
a4					
a5					
a6					
a7	-0.26				
	(-1.8)				
a8					
R^2	0.98	0.81	0.996	0.99	0.99
s	0.01	0.01	0.01	0.02	0.01
Q (6 lags)	4.97	1.69	0.75	14.21*	29.15**
Q (24 lags)	9.62	29.17	19.75	20.94	45.62**
$F(n,d)^{b,c}$	1.03	0.68	1.17	1.05	0.56

Notes: See Table 4-1 for definitions of variables. t-statistics for a given coefficient are in parentheses immediately below the coefficient. ai is the ith autocorrelation coefficient (only autocorrelation coefficients with t-statistics of 1.5 or larger are reported), s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness.

Samples for equations using the DMRN shock concept start in 1958:I.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]Dependent variables in Columns (1) and (2) are as

TABLE 4-11--continued

previously defined in Table 4-1. Others are the GNP-weighted shares of the ratio of the component to GNP. That is, they are of the general form, $(X/Y)\cdot L(Y)$, where X is some component of GNP.

[b]F-statistic is formed by restricting to zero the coefficients of all "relevant" lagged DMRs, where "relevant" is defined as specified on pp. 226-228, above. F(n,d)=F(16,75) except for the following: LUR F(13,75) (3 positive DMRs left unrestricted), GPDI F(15,75), G F(11,75), NRFI F(15,75), STR F(15,75), H F(13,75), DINVY F(15,75), CSERV F(11,75). Corresponding values with F(16,75) (that is, with all lagged DMR coefficients restricted to zero) for these dependent variables are given in Table 4-15.

[c]F(16,76) for the FG equation.

TABLE 4-12: QUARTERLY EQUATIONS FOR UNEMPLOYMENT, REAL GNP, AND THE COMPONENTS OF REAL GNP, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1954:I-1979:III U.S. DATA, "L(X)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF GNP)

	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory Variable[a]	LUR[b]	L(Y)[b]	C	GPDI	G	NETX[c]
Constant			5.15	4.51	2.19	
			(60.2)	(8.3)	(21.6)	
t			0.01	0.01	0.005	
			(150.0)	(23.5)	(50.8)	
LFG			-0.02	-0.17	0.54	
			(-1.0)	(-1.3)	(22.0)	
DMR			0.41	3.01	0.25	
			(2.7)	(2.8)	(1.8)	
DMR1			0.88	5.60	0.23	
			(4.0)	(3.7)	(1.2)	
DMR2			1.10	7.53	0.39	
			(4.5)	(4.5)	(1.7)	
DMR3			1.59	9.32	0.58	
			(5.9)	(5.3)	(2.3)	
DMR4			1.70	8.84	0.53	
			(6.1)	(4.9)	(2.0)	
DMR5			1.71	6.53	0.51	
			(6.2)	(3.7)	(1.9)	
DMR6			1.65	4.35	0.53	
			(6.3)	(2.6)	(2.0)	
DMR7			1.53	3.33	0.61	
			(6.1)	(2.1)	(2.3)	
DMR8			1.16	2.50	0.56	
			(4.8)	(1.6)	(2.1)	
DMR9			1.12	1.28	0.53	
			(4.9)	(0.8)	(2.0)	
DMR10			0.89	-0.88	0.71	
			(4.0)	(-0.6)	(2.9)	
DMR11			0.56	-2.18	0.71	
			(2.6)	(-1.5)	(3.0)	
DMR12			0.50	-2.39	0.78	
			(2.3)	(-1.6)	(3.4)	
DMR13			0.08	-2.49	0.54	
			(0.3)	(-1.7)	(2.4)	
DMR14			-0.04	-1.13	0.56	
			(-0.2)	(-0.8)	(2.7)	
DMR15			-0.06	-0.95	0.42	
			(-0.3)	(-0.7)	(2.1)	
DMR16			-0.06	-0.61	0.10	
			(-0.4)	(-0.6)	(0.7)	

TABLE 4-12--continued

Explanatory Variable[a]	(1) LUR[b]	(2) L(Y)[b]	(3) C	(4) GPDI	(5) G	(6) NETX[c]
a1			0.85	0.83	0.92	
			(7.3)	(7.3)	(8.0)	
a2						
a3						
a4						
a5						
a6						
a7						
a8						
R^2			0.9996	0.98	0.9993	
s			0.006	0.04	0.006	
Q (6 lags)			4.44	0.56	19.11**	
Q (24 lags)			13.31	14.90	37.56*	
F(n,d)[d]			4.91**	2.14*	1.35	

TABLE 4-12--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) H	(11) DINVY[b]
Constant	3.59	3.55	1.61	4.02	
	(10.0)	(7.1)	(4.8)	(6.0)	
t	0.01	0.01	0.01	0.01	
	(24.6)	(20.1)	(22.5)	(12.4)	
LFG	-0.08	-0.25	0.27	-0.30	
	(-0.9)	(-2.1)	(3.4)	(-1.9)	
DMR	0.76	0.94	0.92	2.80	
	(1.7)	(1.5)	(1.6)	(3.0)	
DMR1	1.76	2.30	1.54	5.14	
	(2.4)	(2.3)	(1.9)	(3.6)	
DMR2	3.51	4.20	3.16	6.86	
	(3.9)	(3.4)	(3.4)	(4.1)	
DMR3	5.30	6.94	4.37	6.93	
	(5.2)	(5.1)	(4.3)	(3.7)	
DMR4	5.87	7.89	4.56	4.44	
	(5.4)	(5.5)	(4.3)	(2.2)	
DMR5	5.94	8.05	4.85	1.40	
	(5.2)	(5.4)	(4.6)	(0.7)	
DMR6	5.48	7.30	4.59	0.20	
	(4.9)	(4.9)	(4.6)	(0.1)	
DMR7	5.49	7.73	3.63	-0.63	
	(5.0)	(5.1)	(3.7)	(-0.3)	
DMR8	5.27	6.97	3.82	-0.46	
	(4.8)	(4.6)	(4.1)	(-0.2)	
DMR9	4.45	5.77	3.10	-2.30	
	(4.2)	(4.0)	(3.5)	(-1.3)	
DMR10	3.02	4.33	1.58	-3.26	
	(3.0)	(3.1)	(1.8)	(-1.9)	
DMR11	1.95	3.15	0.41	-3.43	
	(2.0)	(2.4)	(0.5)	(-2.1)	
DMR12	1.60	3.10	-0.47	-3.08	
	(1.7)	(2.5)	(-0.6)	(-1.9)	
DMR13	0.81	1.74	-0.73	-1.84	
	(0.9)	(1.4)	(-0.9)	(-1.2)	
DMR14	0.25	0.83	-0.96	-1.63	
	(0.3)	(0.7)	(-1.2)	(-1.1)	
DMR15	-0.40	0.10	-1.68	-0.64	
	(-0.6)	(0.1)	(-2.2)	(-0.5)	
DMR16	-0.83	-1.17	-0.95	0.15	
	(-1.8)	(-1.8)	(-1.7)	(0.2)	

TABLE 4-12--continued

Explanatory Variable[a]	(7) NRFI	(8) PDUR	(9) STR	(10) H	(11) DINVY[c]
a1	1.19	1.15	0.93	1.06	
	(10.4)	(10.1)	(8.1)	(9.2)	
a2	-0.27				
	(-1.6)				
a3		-0.29			
		(-1.8)			
a4				-0.31	
				(-1.8)	
a5					
a6		-0.33			
		(-2.0)			
a7		0.28			
		(1.6)			
a8		-0.18			
		(-1.6)			
R^2	0.997	0.997	0.99	0.98	
s	0.02	0.03	0.02	0.04	
Q (6 lags)	3.78	3.09	2.28	1.57	
Q (24 lags)	16.00	14.27	12.37	16.77	
F(n,d)[d]	2.94**	3.16**	2.24*	2.12*	

TABLE 4-12--continued

Explanatory Variable[a]	(12) CDUR	(13) CPER	(14) CSERV	(15) FG	(16) SLG
Constant	3.98	4.32	4.02	4.15	2.78
	(7.5)	(46.9)	(64.4)	(83.2)	(11.6)
t	0.01	0.01	0.01	-0.0004	0.01
	(25.7)	(107.3)	(230.4)	(-0.6)	(41.1)
LFG	-0.33	0.03	0.03		0.12
	(-2.6)	(1.2)	(2.3)		(2.0)
DMR	1.23	0.38	0.30	1.07	0.55
	(1.6)	(2.0)	(2.3)	(1.9)	(1.6)
DMR1	3.18	0.87	0.34	2.02	0.46
	(2.9)	(3.3)	(2.1)	(2.5)	(1.0)
DMR2	3.91	1.17	0.52	2.18	0.89
	(3.1)	(3.9)	(3.0)	(2.3)	(1.6)
DMR3	5.13	1.61	0.95	2.69	1.19
	(3.7)	(5.1)	(5.2)	(2.5)	(2.0)
DMR4	5.71	1.73	0.87	3.33	1.04
	(4.0)	(5.3)	(4.7)	(2.9)	(1.7)
DMR5	5.37	1.67	0.93	4.06	0.82
	(3.7)	(5.2)	(5.1)	(3.4)	(1.3)
DMR6	4.64	1.69	0.94	3.67	0.66
	(3.2)	(5.6)	(5.3)	(2.9)	(1.0)
DMR7	3.84	1.54	0.94	3.80	0.70
	(2.7)	(5.3)	(5.4)	(3.0)	(1.1)
DMR8	2.91	1.17	0.66	4.03	0.51
	(2.1)	(4.2)	(3.8)	(3.2)	(0.8)
DMR9	3.33	1.01	0.58	3.83	0.33
	(2.4)	(3.8)	(3.6)	(3.2)	(0.5)
DMR10	1.54	0.98	0.51	3.10	0.62
	(1.2)	(3.8)	(3.2)	(2.6)	(1.1)
DMR11	0.31	0.62	0.42	2.12	0.56
	(0.3)	(2.4)	(2.7)	(1.9)	(1.0)
DMR12	0.93	0.23	0.39	2.13	0.67
	(0.8)	(0.9)	(2.5)	(2.0)	(1.2)
DMR13	-0.10	-0.20	0.13	1.52	0.33
	(-0.1)	(-0.8)	(0.8)	(1.6)	(0.6)
DMR14	-0.33	-0.21	-0.01	1.31	0.46
	(-0.3)	(-0.8)	(-0.1)	(1.5)	(0.9)
DMR15	-0.12	-0.21	-0.06	1.74	0.20
	(-0.1)	(-0.9)	(-0.4)	(2.2)	(0.4)
DMR16	-0.31	-0.10	-0.02	0.38	-0.11
	(-0.4)	(-0.6)	(-0.2)	(0.7)	(-0.3)

TABLE 4-12--continued

Explanatory Variable[a]	(12) CDUR	(13) CPER	(14) CSERV	(15) FG	(16) SLG
a1	0.91	0.85	0.62	0.98	0.88
	(7.9)	(7.4)	(5.3)	(8.5)	(7.6)
a2					
a3					
a4					
a5					
a6					
a7	-0.31				
	(-2.0)				
a8					
R^2	0.995	0.999	0.9998	0.96	0.999
s	0.03	0.008	0.005	0.02	0.01
Q (6 lags)	2.78	5.09	4.90	31.81**	34.89**
Q (24 lags)	8.58	24.37	14.79	44.80**	60.77**
$F(n,d)^{d,e}$	1.60	4.03**	3.42**	1.34	0.56

Notes: See Table 4-1 for definitions of variables.

t-statistics for a given coefficient are in parentheses immediately below the coefficient. ai is the ith autocorrelation coefficient (only autocorrelation coefficients with t-statistics of 1.5 or larger are reported), s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness.

Samples for equations using the DMRN shock concept start in 1958:I.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]Dependent variables of all estimated equations are of the form L(X), where X is some component of GNP.

[b]See Table 4-11 for estimated equation.

TABLE 4-12--continued

[c]This GNP-component contains negative values, so that its log is not defined for all of its values. Therefore, no equation has been estimated for this component.

[d]F-statistic is formed by restricting to zero the coefficients of all "relevant" lagged DMRs, where "relevant" is defined as specified on pp. 226-228, above. F(n,d)=F(16,75) except for the following: STR F(15,75), H F(15,75). Corresponding values with F(16,75) (that is, with all lagged DMR coefficients restricted to zero) for these dependent variables are given in Table 4-15.

[e]F(16,76) for the FG equation.

TABLE 4-13: JOINTLY ESTIMATED MONEY-GROWTH AND REAL OUTPUT EQUATIONS, QUARTERLY DATA, 1954:I-1979:III

	Dependent Variable = L(Y)	
Explanatory Variable	coefficient	asymptotic t-statistic
Constant	5.27	17.8
t	0.01	16.2
LFG	0.11	1.7
DMR	0.76	2.6
DMR1	1.10	2.5
DMR2	1.37	2.7
DMR3	2.66	4.7
DMR4	2.58	4.0
DMR5	1.70	2.5
DMR6	1.45	2.3
DMR7	1.28	2.1
DMR8	0.93	1.6
DMR9	0.90	1.5
DMR10	0.28	0.5
DMR11	0.20	0.4
DMR12	0.32	0.6
DMR13	0.14	0.3
DMR14	0.43	0.9
DMR15	0.12	0.3
DMR16	-0.01	-0.04
a1	0.98	6.2
a2	-0.05	-0.3
a3	-0.16	-0.8
a4	-0.02	-0.1
a5	-0.06	-0.3
a6	0.12	0.6
a7	-0.15	-0.8
a8	0.13	0.9

TABLE 4-13--continued

	Dependent Variable = DM	
Explanatory Variable	coefficient	asymptotic t-statistic
Constant	0.02	14.2
DM1	0.69	5.5
DM2	-0.36	-2.2
DM3	0.22	1.3
DM4	-0.12	-0.8
DM5	0.29	2.5
FEDV	0.02	1.1
LUR1	0.004	2.9

Notes: See Table 4-1 for definitions of variables. ai is the ith autocorrelation coefficient. Parameter estimates are derived using a version of Mishkin's (1983) nonlinear-least squares joint estimation program. See the text for details of estimation procedure.

variables are the GNP-weighted ratios of some component of GNP to GNP. Table 4-12 presents the corresponding equations where dependent variables are the (natural) logs of these components. Results in general are consistent with Hypothesis 1 and the annual findings, but some interesting discrepancies do emerge.

Equations 4-11-3 and 4-12-3 are the equations for total consumption expenditures. There is no evidence that the GNP-weighted ratio of C to Y responds to DMRs with persistence. No lagged shock coefficient is statistically significant in Equation 4-11-3, and the F-test for the joint significance of the 16 lagged shock terms fails to reject the null hypothesis of no significance. However, the log level of consumption expenditures (Equation 4-12-3) responds to shocks with substantial persistence: DMR1 through DMR7 (plus DMR) are positive and statistically significant, and the F-test for joint significance of these seven shock terms rejects the null hypothesis at the one percent level. Thus, in the quarterly data, Barro-type shocks cause total consumption to respond with "weak persistence." This is somewhat in harmony with annual results for consumption, which narrowly miss exhibiting "weak persistence." It is also in harmony with Hypothesis 1, which restricts total consumption expenditures not to respond to shocks with "strong persistence."

Equations 4-11-4 and 4-12-4 are the equations for gross private domestic investment. As previously discussed, the annual work separates GPDI into NPDI and DEP while the quarterly work does not, a state of affairs which prevails for all the investment concepts investigated in this study. This is on the

grounds that the National Income and Product Accounts data do not separate net investment from depreciation expenditures in the quarterly data. Thus, quarterly results for investment will not be strictly comparable with annual results. However, an advantage of this is that it will allow some exploration of the extent to which results are robust to the inclusion or exclusion of replacement investment.

On balance, the GPDI equations are consistent with Hypothesis 1, revealing "strong persistence" in response to Barro-type money shocks. However, the magnitude of this response can be questioned. The F-test for the joint significance of all lagged DMRS except those which are both negatively-signed and statistically significant in Equation 4-11-4 is significant only at the five percent level, as is the case in the analogous equation for L(GPDI). For both equations, the effect of DMRs begins with the period of the shock, reaches a peak at three quarters, and continues to have a statistically significant impact for six or seven quarters.

Equations 4-11-5 and 4-12-5 are the total government-expenditures equations. Beginning with Equation 4-11-5, the F-test for the joint significance of all lagged DMRs except those which are both negatively-signed and statistically significant (five percent level)[17] fails to reject the null hypothesis of no joint significance, so that there is no evidence that shocks

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[17]Henceforth such restrictions will be referred to as restricting all "relevant" lagged DMRs to zero. The discussion on pp. 226-228, above, has an elaboration of the exact meaning of the restriction.

raise the GNP-weighted ratio of G to Y. Equation 4-12-5, the L(G) equation, exhibits an anomaly that is occasionally observed in the present study (and which is also observed to a lesser extent in Equation 4-11-5). t-statistics indicate that a long lag of DMRs--specifically, DMR3 through DMR15--are both positive and statistically significant. However, the F-test for the joint significance of all lagged shocks fails to reject the null hypothesis of no significance at the five percent level. The likely explanation of this phenomenon has been described by Haraf, who encounters it in his own research. Haraf points out that

> the apparent conflict between the joint and individual tests (that is, between the F-test and the t-tests) arises because, even though the lagged DMR are uncorrelated, either differencing or applying a second order autocorrelation correction (as Barro and Rush do) makes the covariance matrix of the parameter estimates nondiagonal, in this case highly so. This makes the examination of individual 't-ratios' inappropriate when considering the above hypothesis, since they represent correlated tests--a type I error on one coefficient, for example, increases the probability of a type I error on other coefficient estimates which are positively correlated. (Haraf, 1983, p. 114)

Presumably the problem is exacerbated by the use of an eighth-order adjustment for serial correlation, as is done in the quarterly work of this study. It thus can be concluded on the basis of Equations 4-11-5 and 4-12-5 that total government expenditures do not respond to Barro-type money shocks with "strong persistence."

Equation 4-11-6 is the equation for net exports. Results indicate that Barro-type shocks cause NETX to fall relative to

GNP. Again, an absence of "strong (positive) persistence" characterizes the specification.

In sum, Equations 4-11-3 through 4-11-6, and 4-12-3 through 4-12-6, support Hypothesis 1. Only GPDI gives evidence of responding to Barro-type shocks with "strong persistence" in the quarterly data. Thus Hypothesis 1 is consistent with both the quarterly and the annual data, when Barro's money-growth forecasting equation is used to generate shocks and a two-stage estimation procedure is employed.

Results for the components of GNP: Hypothesis 2

Equations 4-11-7 through 4-11-10, and 4-12-7 through 4-12-10, are the disaggregated investment equations, which are relevant to investigating Hypothesis 2 (the changes-in-inventories equation--Equation 4-11-11--will be discussed below in the context of Hypothesis 4). Results are best summarized as being consistent with Hypothesis 2. Equations 4-11-7 and 4-12-7 are the nonresidential fixed investment equations. $(NRFI/Y)\cdot L(Y)$ shows persistence beginning at two quarters, peaking at seven quarters, and continuing through 11 quarters. The failure of either DMR or DMR1 to exhibit persistence is interesting on time-to-build premises, since such a pattern is to be expected of categories with longer production periods, where several periods might elapse between the decision to construct a project and the date at which construction would begin in earnest. The F-statistic testing for joint significance of 15 "relevant" lagged DMRs rejects the null hypothesis of no significance at the one

percent level. Similar results emerge for the L(NRFI) equation, although here DMR1 is significant.

The equations stemming from the disaggregation of NRFI are Equations 4-11-8 and 4-12-8, for producers' durable expenditures, and 4-11-9 and 4-12-9, for nonresidential structures. The PDUR equations exhibit the most persistence of any investment category, a result also found in the annual work. Shocks begin having a significant impact on PDUR with DMR1, continue to a peak impact with DMR7 for the ratio and DMR5 for the log level, and continue to have a significant impact through DMR12. F-tests for joint significance are significant at the one percent level. Again as was true for the annual results, the exceptionally strong response of PDUR to shocks is at variance with Hypothesis 2, which predicted that nonresidential structures ought to show the most persistence.

Equations 4-11-9 and 4-12-9 are the equations for nonresidential structures. The outstanding result emerging is the weakness of the response of STR to lagged shocks. The F-test for the GNP-weighted ratio of STR to Y fails to reject the null hypothesis of no significance for 15 "relevant" lagged DMRs. This finding is observed despite the significance of a substantial number of individual DMR coefficients: Statistical significance begins at DMR2, peaks in magnitude of effect at DMR6, and continues through DMR9. However, as pointed out by Haraf (above, p. 269), these individual coefficients are statistically irrelevant given the failure of the F-test to reject the null hypothesis. A similar pattern of response by

individual coefficients is observed for the L(STR) equation, although there the F-test for joint significance is significant at the five percent level. Despite the somewhat better results for L(STR), it is clear that nonresidential structures do not exhibit an unambiguous response of "strong persistence" to Barro-type money shocks over the 1954-79 sample. This is in conflict with the annual results, which did show such significance over this sample, but it confirms the general conclusion made for the annual data that the response of STR was substantially weaker than that predicted by Hypothesis 2. The behavior of nonresidential construction continues in the quarterly work to raise questions concerning the viability of the time-to-build propagation mechanism.[18]

In contrast to the equations for nonresidential structures, the equations for residential structures (Equations 4-11-10 and 4-12-10) are consistent with Hypothesis 2. It will be recalled that, in the annual equations, the contemporaneous value of DMR had a statistically significant impact on H, but no lagged DMR had such an impact, a result consistent with the predictions of time-to-build to be sure, but also easily explained in other ways. However, the quarterly results show exactly the kind of moderate intrayear persistent response by H that one would

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[18]Reestimating the two STR equations over samples starting in 1957:I and 1960:I, respectively, basically leaves the performance of the equations unchanged. Accordingly, it seems unlikely that the poor results in the quarterly data can be attributed to the tax changes of the 1953-54 period (discussed above, pp. 229-232). This finding in turn raises questions about the tax-related explanation of the problems in the annual data for STR over the 1946-85 sample.

predict on the basis of time-to-build (and which is formalized in Hypothesis 2), given what is known about average periods of production for this category. The GNP-weighted ratio of H to GNP shows three quarters of persistence (plus the statistical significance of DMR), with the effect peaking at the second quarter following the shock's occurrence. The L(H) equation shows a similar pattern but with persistence extending to four quarters. For both equations, the F-test for the joint significance of all "relevant" DMRs rejects the null hypothesis of no significance at the five percent level. Thus H exhibits "strong persistence" in response to shocks. These results are thoroughly consistent with Hypothesis 2.

In sum, the quarterly results are consistent with Hypothesis 2, with a single major exception. The weak response of nonresidential structures, previously observed in the annual work, continues to be observed in the quarterly data. However, the failure of STR to show "strong persistence" is the result solely of the F-test for joint significance of all "relevant" shocks failing to reject the null hypothesis of no joint significance. A number of individual t-statistics are significant in both STR equations, and in the L(Y) equation the F-test is significant as well. Thus, nonresidential structures does not act so as to imply a strong rejection of Hypothesis 2.

Results for the components of GNP: Hypothesis 3

Hypothesis 3, which restricts the behavior of the components of total consumption and total government expenditures, can be investigated via inspection of Equations 4-11-12 through 4-11-16,

and 4-12-12 through 4-12-16. Results are largely consistent with Hypothesis 3, although some questions may be raised about the response of total government spending to Barro-type money shocks.

Equations 4-11-12 through 4-11-14 and 4-12-12 through 4-12-14 are results stemming from the disaggregation of total consumption expenditures into durable expenditures (CDUR), perishable-goods expenditures (CPER), and expenditures by consumers on services (CSERV). There is no evidence that any of these categories exhibits "strong persistence" in response to money shocks, although both CPER and CSERV display "weak persistence" (the F-test on the log-level form of these equations is significant at the one percent level).

Equations 4-11-15, 4-11-16, 4-12-15, and 4-12-16 are the equations for federal government expenditures (FG) and state-and local-expenditures (SLG). It is more difficult to interpret these equations than it is the consumption equations, because of their rejection of the hypothesis of no serial correlation in the residuals (one or more Q-statistic is significant in all four equations). As measured by F-statistics testing for the joint significance of all lagged shocks, the evidence is that neither FG nor SLG responds with persistence to Barro-type money shocks. This conclusion is confirmed by individual coefficients in the SLG equations, which reveal little effect of such shocks. The considerable number of lagged DMRs which are statistically significant and positive in the FG equations can be dismissed on

the grounds previously discussed (p. 269, above). In sum, on balance the quarterly analysis is consistent with Hypothesis 3.

Quarterly Results Using Alternative Measures of Nominal-Demand Shocks

Table 4-14 presents F-statistics testing for the joint significance of all "relevant" lagged nominal-demand shocks, for the seven shock concepts described in the discussion accompanying Tables 4-3 and 4-13.[19] All estimation except for that utilizing DMRN is for the 1954:I-1979:III period. The sample for equations using DMRNs as explanatory variables begins in 1958:I, because 16 additional degrees of freedom are lost due to the joint estimation procedure used to generate the DMRNs (Table 4-13, above). In general, empirical results confirm the findings reported previously when Barro-type money shocks are used as explanatory variables in the quarterly work.

The results for unemployment and real GNP are presented in the first two columns of Table 4-14. Results strongly confirm the findings previously derived using DMRs that both unemployment and real output respond to shocks with "strong persistence." Six out of seven shock concepts generate an F-test that rejects the null hypothesis of no (positive) persistence at the five percent level, and five out of seven reject the hypothesis at the one percent level. An important characteristic of the results for LUR and L(Y) is the considerable persistence induced by nominal-

[19]Table 4-15 presents F-statistics testing for the joint significance of all 16 lagged nominal-demand shocks, for all those equations in Table 4-14 where one or more lagged DMRs are left unrestricted.

TABLE 4-14: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	LUR	L(Y)	C	
DMR	(4.27)**	2.49**	0.60	4.91**
DMRG	2.24*	1.94*	(0.91)	3.05**
DMRM	2.87**	2.62**	(0.84)	1.24
DMRN	(3.97)**	2.51**	0.42	4.31**
DBR	1.54	0.97	1.17	1.17
DYRG	2.61**	5.89**	(0.85)	2.18*
DYRM	3.19**	4.97**	(3.66)**	2.05*

TABLE 4-14--continued

Shock Concept	GPDI		G		NETX
DMR	(2.21)*	2.14*	(1.30)	1.35	1.93*
DMRG	(2.45)**	2.07*	(1.67)	0.84	1.62
DMRM	4.05**	3.69**	(1.75)	0.83	2.39**
DMRN	(3.07)**	(2.69)**	(6.05)**	1.99*	1.93*
DBR	1.13	1.11	0.56	3.36**	0.75
DYRG	4.08**	4.31**	(1.78)	1.02	1.10
DYRM	(3.37)**	3.37**	(1.14)	0.88	1.74

TABLE 4-14--continued

Shock Concept	NRFI		PDUR		STR	
DMR	(2.45)**	2.94**	2.52**	3.16**	(1.67)	(2.24)*
DMRG	2.40**	2.49**	(2.30)*	2.72**	0.67	0.99
DMRM	3.97**	3.81**	3.42**	3.37**	1.12	1.60
DMRN	(3.93)**	(4.83)**	(2.68)**	(3.85)**	(2.52)**	2.99**
DBR	2.17*	2.81**	1.20	1.85*	1.55	1.76
DYRG	1.67	2.53**	1.10	1.72	1.10	1.34
DYRM	2.35**	2.89**	1.78	2.35**	(1.40)	(1.53)

TABLE 4-14--continued

Shock Concept	H		DINVY
DMR	(2.42)**	(2.12)*	(1.13)
DMRG	(1.85)*	(1.82)*	(1.44)
DMRM	(4.60)**	(4.26)**	1.81*
DMRN	(3.00)**	(2.75)**	0.67
DBR	1.22	1.08	0.86
DYRG	0.97	1.22	3.39**
DYRM	(1.22)	(1.15)	(2.88)**

TABLE 4-14--continued

Shock Concept	CDUR		CPER		CSERV	
DMR	1.03	1.60	0.68	4.03**	(1.17)	3.42**
DMRG	0.98	1.49	(0.96)	1.18	(1.27)	1.19
DMRM	0.80	1.11	(0.76)	0.64	(1.31)	0.78
DMRN	0.99	1.72	0.68	2.65**	(1.23)	4.54**
DBR	0.37	0.39	1.19	1.19	1.02	3.41**
DYRG	1.80	2.11*	(0.73)	1.98*	(1.60)	1.35
DYRM	2.34**	2.55**	(1.75)	1.70	(1.71)	1.32

TABLE 4-14--continued

Shock Concept	FG		SLG	
DMR	1.05	1.34	0.56	0.56
DMRG	0.61	0.80	0.79	0.34
DMRM	0.84	0.91	(1.28)	0.42
DMRN	0.61	0.80	2.16*	1.88*
DBR	1.67	1.68	0.45	1.03
DYRG	5.33**	3.13**	1.08	0.77
DYRM	2.29*	1.99*	0.84	0.80

Notes: See Table 4-1 for definitions of variables. For LUR and L(Y), F-statistics are formed from regressions where the forms of dependent variables are as defined in Table 4-1. For remaining F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the

TABLE 4-14--continued

dependent variable is of the general form L(X), where, in both cases, X is the component of GNP indicated. For NETX and DINVY, some values of L(X) are undefined, so that only left-hand-side Fs appear for these GNP-components.

Where all lagged shocks are restricted to zero, for equations using all shock concepts except DMRN F-statistics are F(16,75) (F(16,76) for FG equations) and effective sample period is 1954:I-1979:III. For Fs stemming from equations using DMRN as shock concept, F-statistics are F(16,59) (F(16,60) for FG equations) and effective sample period is 1958:I-1979:III.

For the definition of "relevant" in the present context, see the discussion on pp. 226-228, above. F-statistics in parentheses leave one or more lagged shocks unrestricted; others restrict all lagged shocks to zero. Following is a list of: (a) F-statistic degrees of freedom, and, (b) lagged shocks left unrestricted, for all Fs in parentheses (presented in the general form F(n,d) {·}, where within brackets are the numbers corresponding to the DMR lags left unrestricted). That is, the first entry below, for LUR, means: F degrees of freedom are 13 in numerator and 75 in denominator, and DMR13, DMR14, and DMR15 were left unrestricted (since their coefficients were both "wrongly-signed" and significant at the five percent level).

First, for left-hand-side Fs: for DMR: LUR F(13,75),{13-15}; GPDI F(15,75),{12}; G F(11,75),{1-5}; NRFI F(15,75),{16}; STR F(15,75),{15}; H F(13,75),{10-12}; DINV F(15,75),{11}; SERV F(11,75),{1-5}.

For DMRG: C F(12,75),{1-4}; GPDI F(13,75),{10-12}; G F(12,75),{1-4}; PDUR F(15,75),{16}; H F(14,75),{9,10}; DINVY F(14,75),{10,11}; CPER F(12,75),{1-4}; CSERV F(12,75),{1-4}.

For DMRM: C F(5,75),{1-9,11,12}; G F(11,75),{1-5}; H F(8,75),{7-14}; CPER F(6,75),{1-9}; CSERV F(9,75),{1-6,8}; SLG F(11,75),{1-5}.

For DMRN: LUR F(15,59),{15}; GPDI F(14,59),{12,13}; G F(11,59),{1-5}; NRFI F(14,59),{15,16}; PDUR F(15,59),{16}; STR F(15,59),{16}; H F(11,59),{10-14}; CSERV F(12,59),{1-4}.

For DBR: none.

For DYRG: C F(12,75),{1-3,15}; G F(14,75),{1,2}; CPER F(13,75),{1-2,15}; CSERV F(12,75),{1-3,14}.

For DYRM: C F(13,75),{1-2,15}; GPDI F(15,75),{8}; G F(14,75),{2}; STR F(13,75),{11-13}; H F(11,75),{3-7}; DINVY F(13,75),{6-8}; CPER F(13,75),{1,15,16}; CSERV F(13,75),{1-3}.

Second, for right-hand-side Fs: for DMR: STR F(15,75),{15}; H F(15,75),{11}. For DMRG: H F(14,75),{9,10}. For DMRM: H F(10,75),{9-14}. For DMRN: LUR F(15,59),{15}; GPDI F(15,59),{13}; NRFI F(15,59),{16}; PDUR F(15,59),{16}; H F(12,59),{10-12,14}. For DBR: none. For DYRG: none. For DYRM: STR F(14,75),{11,12}; H F(12,75),{4-7}.

F-statistics corresponding to the above which restrict all 16 lagged shocks to zero are presented in Table 4-15.

TABLE 4-14--continued

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-15: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	LUR	L(Y)	C
DMR	3.64**		
DMRG			1.06
DMRM			1.80*
DMRN	3.95**		
DBR			
DYRG			3.42**
DYRM			4.37**

TABLE 4-15--continued

Shock Concept	GPDI		G	NETX
DMR	2.08*		1.44	
DMRG	2.12*		1.66	
DMRM			2.24*	
DMRN	2.73**	2.55**	5.53**	
DBR				
DYRG			2.59**	
DYRM	3.18**		1.79	

TABLE 4-15--continued

Shock Concept	NRFI		PDUR		STR	
DMR	2.51**				1.65	2.22*
DMRG			2.48**			
DMRM						
DMRN	4.38**	4.78**	3.08**	4.12**	2.40**	
DBR						
DYRG						
DYRM					1.34	1.46

TABLE 4-15--continued

Shock Concept	H		DINVY
DMR	2.07*	1.99*	1.11
DMRG	1.94*	1.83*	1.56
DMRM	3.12**	3.34**	
DMRN	2.18*	2.13*	
DBR			
DYRG			
DYRM	1.37	1.32	2.55**

TABLE 4-15--continued

Shock Concept	CDUR	CPER	CSERV
DMR			1.49
DMRG		1.05	1.45
DMRM		1.52	3.02**
DMRN			1.45
DBR			
DYRG		2.15*	5.67**
DYRM		2.36**	3.95**

TABLE 4-15--continued

Shock Concept	FG	SLG
DMR		
DMRG		
DMRM		1.32
DMRN		
DBR		
DYRG		
DYRM		

Notes: See Table 4-1 for definitions of variables. For LUR, F-statistics are formed from regressions where the form of dependent variable is as defined in Table 4-1. For remaining F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$,

TABLE 4-15--continued

and the right-hond-side Fs are those from a regression where the dependent variable is of the general form L(X), where, in both cases, X is the component of GNP indicated. For DINVY, some values of L(X) are undefined, so that only left-hand-side Fs appear for this GNP-component.

All Fs restrict all 16 lagged shocks to zero (omitted Fs are reported in Table 4-14). For equations using all shock concepts except DMRN, F-statistics are F(16,75) and effective sample period is 1954:I-1979:III. For Fs stemming from equations using DMRN as shock concept, F-statistics are F(16,59) and effective sample period is 1958:I-1979:III.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

GNP shocks, a finding which did not characterize the annual work with such shocks. The notable exception to the tendency for persistence to be robust across shocks is the case of monetary-base shocks, which, in the quarterly data, fail to elicit a persistent response from either unemployment or output. For this last case, inspection of the regression equations (not reported in this study) associated with these F-statistics reveals coefficient patterns on DBRs for LUR and L(Y) that are similar to other patterns; however, individual coefficients do not tend to be statistically significant. The anomalous results derived using Rush-type monetary base shocks is consistent with the result previously derived using such shocks for annual GNP, but conflicts both with the result previously derived for annual unemployment and with the results reported for unemployment in Rush (1986).

Turning now to the consistency of results with Hypothesis 1 over the seven shock concepts, results confirm the previous findings that "strong persistence" is confined to the investment portion of GNP. Excepting the DBRs, all shocks generate "strong persistence" by GPDI; moreover, all five of these supplementary shock concepts generate more persistence than do the DMRs (as measured by F-tests). All five of these shocks generate F-statistics which are significant at the one percent level, as opposed to the F for the DMRs being significant only at the five percent level. The results for consumption also are, on balance, consistent with Hypothesis 1: While consumption shows widespread evidence of exhibiting "weak persistence" in response to the

seven shock concepts, only one of the seven (Mishkin-type nominal-GNP shocks) generates a response of "strong persistence." Thus results for consumption are consistent with Hypothesis 1 for six out of seven shock concepts. Results for government expenditures and net exports also are consistent with Hypothesis 1. A "weakly persistent" response of G is elicited by DBR, and "strong persistence" by G is generated by DMRN. However, G does not respond with any persistence to five of seven shock concepts. Nominal-demand shocks show some explanatory power over the behavior of NETX in the quarterly data. However, inspection of the regressions from which the F-statistics were derived (not included in this study) reveals that only Mishkin-type money shocks induce a response from NETX that is both positive and significant. Thus there is little evidence that shocks cause the GNP-weighted ratio of NETX to GNP to rise.

The disaggregation of GPDI supports Hypothesis 2, with the notable exception of nonresidential structures, a component which continues to exhibit its aberrant behavior. Nonresidential fixed investment shows a somewhat stronger and more robust response to shocks than even GPDI: Six out of seven shock concepts generate a "strongly persistent" response of this category, and the remaining shock concept (DYRG) exhibits "weak persistence." In fact, NRFI shows more of a persistent response than do either of its components. Producers' durable expenditures continues to show widespread "strong persistence," indicating that the response of this category also is highly robust to variation in shock concept. Nonresidential structures continues to respond

with much less persistence than implied by the time-to-build propagation mechanism. Only one out of seven shock concepts (DMRN) elicits a response of "strong persistence" from this category. The likely explanation for the strength of the response to DMRN is not the joint estimation procedure used to derive DMRN, but rather the fact that the effective sample for the DMRN equation begins in 1958:I rather than 1954:I (the sensitivity of STR to the inclusion of early 1950s data has previously been observed in the annual work, although there "strong persistence" was observed over a sample beginning in 1954). Finally, the response of residential structures is consistent with Hypothesis 2 for four out of seven nominal-demand shock concepts and for four out of five money shock concepts. Moreover, the statistical significance of individual shock coefficients in these equations is fairly short term--as predicted by time-to-build--typically no shock older than DMR4 is both positive and statistically significant. There is no evidence, however, that the "strongly persistent" response of H to money shocks is robust to a change of shock concept to nominal-GNP shocks.

It remains to discuss the robustness of those previous findings which were consistent with Hypothesis 3. The disaggregation of total consumption expenditures leads to results which are thoroughly consistent with the hypothesis. None of the seven shock concepts generates a response of "strong persistence" by CPER or CSERV, and only one out of seven shock concepts (DYRM) elicits such a response from CDUR. A similar finding holds for

state-and-local government expenditures when total government expenditures are disaggregated: Only one of seven shock concepts (DMRN) generates a response of "strong persistence" by SLG (and that only at the five percent level), and no other shock concept generates any persistence at all for this category. The results for federal government expenditures are more open to interpretation. F-tests reveal a clear discrepancy between results for money shocks and for nominal-GNP shocks: the former tending not to generate persistence, and the latter generating "strong persistence," particularly so for Gordon-type nominal-GNP shocks. Thus, the results for FG are best summarized as weakly supporting Hypothesis 3.

Further Results Stemming from the Disaggregation of Real GNP: Blinder-Fischer versus Time-To-Build (Hypotheses 4 and 5)

The previous section has established the overall consistency with the time-to-build mechanism of the response to nominal-demand shocks of the components of GNP. However, these findings do not directly relate to the issue of determining the relative explanatory powers of time-to-build on the one hand, and the Blinder-Fischer inventories-based propagation mechanism on the other. Results discussed in the previous section are consistent with the assertion that the Blinder-Fischer mechanism plays the major role in generating GNP-persistence, or, for that matter, even with the possibility that the dynamics of inventory adjustment are ultimately responsible for the persistence of the fixed investment accounts. An assessment of these possibilities requires addressing the issue of the persistence of changes in

inventories, not only alone but also within the broader context of the GNP-persistence question. These issues--previously summarized as Hypotheses 4 and 5--are addressed in the present section.

Annual Results Using Barro-Type Money Shocks

Annual results relevant to the investigation of Hypothesis 4 are presented in Table 4-6, while results relevant to Hypothesis 5 are presented in Table 4-16. For Hypothesis 4, analysis involves comparing the explanatory power of Barro-type money shocks in a changes-in-inventories equation with that of the other equations stemming from the disaggregation of GNP. In the case of Hypothesis 5, analysis involves subtracting out of GNP those portions of GNP in which persistence is concentrated if one or the other propagation mechanism is "correct," and then exploring the extent of the persistence exhibited by the remainder.

Hypothesis 4 holds that, if Blinder-Fischer is the essential propagation mechanism accounting for the persistence of GNP, then, first, changes in inventories ought to show substantial (positive) persistence, and, second, other components of GNP should not show such persistence. Equation 4-6-12 is the annual changes-in-inventories (DINVY) equation formed using Barro-type money shocks as explanatory variables. The result indicates that the first part of Hypothesis 4 is not contradicted by the data: The DINVY equation unambiguously exhibits "strong persistence." DMR1 has a statistically significant and positive effect, and the F-test for the joint significance of all "relevant" lagged DMRs

rejects the null hypothesis of no joint significance at the one percent level. The negative and significant coefficient on DMR2 is consistent with what would be expected on Blinder-Fischer premises: In the last phase of the adjustment of inventory stocks to a (positive) shock, stocks should be rising at a decreasing rate, meaning that the change in stocks ought to be declining, as is observed for DMR2. Other characteristics of Equation 4-6-12 also are adequate, although the R^2 for the equation is the lowest of that for any of the GNP-component equations. From Equation 4-6-12, in sum, it is clear that one cannot reject the Blinder-Fischer propagation mechanism on the basis of the behavior of DINVY.

The second part of Hypothesis 4 holds that components of GNP other than changes in inventories should not show substantial persistence. However, as Table 4-6 shows, and as has been detailed in the previous section, results do not support this aspect of the hypothesis; in particular, the fixed investment components of GNP, particularly producers' durable expenditures, display substantial persistence. Therefore the annual data do not support the hypothesis that only the Blinder-Fischer mechanism is responsible for the persistence of GNP (at least when Barro-type shocks are used). Of course, it is also the case that the data do not unambiguously support the hypothesis that only the time-to-build mechanism is responsible for the persistence of GNP, since not only the fixed investment accounts but also changes in inventories show a response of "strong persistence" to Barro-type shocks. However, this is less of a

contradiction for "pure" time-to-build than for "pure" Blinder-Fischer, since the time-to-build mechanism suggests that some components of inventory changes ought to exhibit persistence (Chapter 3, p. 134), while, in contrast, it is not apparent why Blinder-Fischer should bring about a persistent response by fixed investment.

Discussion will now turn to Table 4-16 and Hypothesis 5. The main idea is to subtract out of output a particular category of interest (call it X) and see if an appropriate function of the resulting remainder (Y-X) shows a persistent response to money shocks. To test for the relevance of the time-to-build propagation mechanism to explaining Barro's results, X should be set equal to the sum of all categories with average production periods long enough to be subject to the time-to-build effect. While an exact measurement of X is difficult, a good approximation (as discussed above, Chapter 3, p. 109) is net fixed investment; that is, the sum of NRFI and H. To test for the relevance of the inventories-based propagation mechanism, X is set equal to changes in inventories.

Interpreted in this fashion, Table 4-16 constitutes evidence favorable to the time-to-build-based propagation mechanism and unfavorable to the inventories-based mechanism of Blinder and Fischer. Equations 4-16-1 and 4-16-2 are two equations where the dependent variable is of the general form L(Y-X), where X is, respectively, NRFI+H, and DINVV. Equation 4-16-1 indicates that the log of the non-fixed-investment component of GNP fails to show a persistent response to Barro-type shocks, suggesting that,

TABLE 4-16: ANNUAL EQUATIONS FOR Y-NRFI-H AND Y-DINVY, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1946-85 U.S. DATA

Explanatory Variable	(1) L(Y-NRFI-H)	(2) L(Y-DINVY)
Constant	1.53 (55.4)	1.69 (66.8)
t	0.004 (32.8)	0.004 (29.5)
LFG	0.03 (4.7)	0.01 (2.3)
DMR	-0.22 (-2.8)	0.01 (0.1)
DMR1	-0.16 (-1.8)	-0.50 (-0.6)
DMR2	0.02 (0.2)	0.23 (3.0)
DMR3	0.03 (0.4)	-0.30 (-0.5)
a1	0.41 (2.5)	0.18 (1.0)
a2	-0.38 (-2.3)	0.04 (0.2)
R^2	0.99	0.99
s	0.01	0.01
Q (6 lags)	10.10	0.77
F(3,31)	1.33	4.12*

TABLE 4-16--continued

Explanatory Variable	(3) [(Y-NRFI-H)/Y]·L(Y)	(4) [(Y-DINVY)/Y]·L(Y)
Constant	4.20 (20.6)	5.13 (30.4)
t	0.03 (32.1)	0.03 (34.4)
LFG	0.16 (3.6)	0.08 (2.1)
DMR	-1.56 (-2.8)	0.09 (0.2)
DMR1	-1.05 (-1.7)	-0.30 (-0.6)
DMR2	0.23 (0.4)	1.74 (3.2)
DMR3	0.26 (0.4)	-0.15 (-0.3)
a1	0.46 (2.8)	0.14 (0.8)
a2	-0.40 (-2.4)	-0.01 (-0.1)
R^2	0.99	0.99
s	0.05	0.05
Q (6 lags)	9.68	0.84
F(3,31)	1.26	4.37*

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. F-statistics are formed by restricting coefficients on the three lagged shock terms to zero.

*Estimate is statistically significant at the five percent level.

in the annual data, all of the statistically important persistence of Y is accounted for by the behavior of fixed investment. By contrast, Equation 4-16-2 continues to exhibit a positive and persistent response to Barro-type shocks, due to the continued statistically significant impact of DMR2 (however, it is interesting that subtracting DINVY out of GNP does eliminate the statistical significance of DMR1, despite DMR1's being significant in many of the fixed investment equations). The F-test for the joint significance of all three lagged shock terms rejects the null hypothesis of no joint effect at the five percent level.

Equations 4-16-3 and 4-16-4 are two equations where the dependent variable is of the general form $[(Y-X)/Y]\cdot L(Y)$, where again X is, respectively, NRFI+H and DINVY. As has previously been shown in the discussion accompanying the development of Hypothesis 5 (Chapter 3, p. 159), a t-test on a lagged shock coefficient in such an equation essentially amounts to a test of the null hypothesis that the effect of this shock on $(X/Y)\cdot L(Y)$ does not differ significantly from its effect on $L(Y)$. Further, an F-test of the null hypothesis that all lagged shocks are jointly insignificant amounts to a test that the pattern of coefficients on $(X/Y)\cdot L(Y)$ is identical to that of $L(Y)$, a strong result, as it would suggest that all of the persistence in Y is accounted for by that of X.

Viewed from this perspective, Equation 4-16-3 (the Y-NRFI-H equation) is quite favorable towards the hypothesis that time-to-build is the only propagation mechanism of any consequence at

work in the data. First, no lagged money-shock coefficient is statistically significant in the equation. Second, the F-test for the joint significance of the three lagged shocks is insignificant, so that the data cannot reject the null hypothesis that the pattern of effect of lagged DMRs on L(Y) is identical to the pattern of effect on [(NRFI+H)/Y]·L(Y). Finally, the F-test also can be interpreted as a test of the null hypothesis that lagged money shocks do not jointly cause Y-NRFI-H to rise relative to Y. Since this hypothesis also cannot be rejected, the evidence suggests that if those categories most affected by time-to-build conditions are subtracted out of output, what remains does not exhibit a persistent response to money shocks. Equation 4-16-3 thus favors the time-to-build propagation mechanism.

In the Y-DINVY equation (Equation 4-16-4), DMR2 is positive and statistically significant, suggesting that not all (positive) persistence is eliminated by subtracting inventory changes out of output. Further, the F-test for the joint significance of the three lagged DMRs is significant at the five percent level. Not only is the pattern of DMR coefficients different for [(Y-DINVY)/Y]·L(Y) than for L(Y), but also the total effect of such shocks is to increase [(Y-DINVY)/Y]·L(Y). That is, even after inventories are subtracted out of output, what remains continues to increase its share relative to output in response to lagged money shocks. This result is inconsistent with the view that an inventories-based mechanism is the only thing generating

persistence in the real output equation, and it suggests, at minimum, that other important mechanisms exist in the data.[20]

Annual Results Using Alternative Measures of Nominal-Demand Shocks

Annual results using alternative nominal-demand shocks as explanatory variables in general support the conclusions reached using Barro-type shocks. The relevant information for evaluating Hypothesis 4 is presented in Table 4-9. Turning to the F-statistics stemming from the various DINVY equations--where Fs are formed by restricting all "relevant" lagged shocks to equal zero--results are best interpreted as constituting, at most, weak support for Blinder-Fischer. While three out of six money-shock concepts generate a response by DINVY of "strong persistence," in each case the hypothesis of no joint significance is rejected only at the five percent level. Further, there is no evidence that nominal-GNP shocks elicit a persistent and positive effect on the GNP-weighted ratio of DINVY to GNP. By contrast, both PDUR and NRFI respond to shocks with both a stronger, and a more

[20]It is somewhat difficult to interpret Equations 4-16-1 and 4-16-3 in light of the fact that, as revealed in Tables 4-6 and 4-7, nonresidential structures--a key component of NRFI--does not respond to money shocks in a way consistent with the time-to-build hypothesis over the 1946-85 sample. However, above it also was seen that the problems with nonresidential structures may be related to changes in tax policy in the early 1950s and/or the Federal Reserve-Treasury Accord, and Equation 4-6-10 revealed that, if estimated over the 1954-85 period, nonresidential structures responds to money shocks in a way consistent with time-to-build. These facts suggest that a useful check on the correctness of the interpretation of Equation 4-16-3 is to reestimate it over the 1954-85 sample. This was in fact done, with results that are essentially the same as those of Equation 4-16-3. Accordingly, there is no indication that the above is a special case stemming from the poor performance of the nonresidential structures equation over the 1946-85 sample.

prevalent, persistence, while the response of STR over the 1954-85 sample is about as impressive as that of DINVY. These findings are on the whole consistent with the hypothesis that Blinder-Fischer plays a role in generating the persistence of inventory changes, but the results are difficult to reconcile with the idea that the bulk of (annual) GNP-persistence can be attributed to the Blinder-Fischer effect.

Turning now to Hypothesis 5, Table 4-17 explores the robustness to changes in shock concept of the result found previously that subtracting inventory changes from GNP creates a category which continues to respond to Barro-type shocks with persistence, while subtracting fixed investment out of output creates a category which does not respond with persistence. The evidence of Table 4-17 supports this previous finding. Results are strongest for the right-hand-side columns, where dependent variables are of the general form L(Y-X). Only Mishkin-type nominal-GNP shocks elicit a persistent response out of the dependent variable once fixed investment is subtracted from GNP, and this significance is only at the five percent level. In sharp contrast, four of six money-shock concepts, and six of eight nominal-demand-shock concepts, generate a persistent response from L(Y-DINVY).

For the left-hand-side columns, where Fs are formed from equations where the dependent variable is of the general form $[(Y-X)/Y]\cdot L(Y)$, two out of five money shocks, and four out of seven nominal-demand shocks, generate a rise in Y-DINVY relative to output. By contrast, none of the five money shocks causes

TABLE 4-17: F-STATISTICS FROM Y-NRFI-H AND Y-DINVY EQUATIONS, TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, ANNUAL DATA, VARIOUS PERIODS FROM 1946-85, EIGHT SHOCK CONCEPTS

Shock Concept	Y-NRFI-H		Y-DINVY	
DMR	1.26	1.33	4.37*	4.12*
DMRGA	0.28	0.35	2.61	(5.12)**[a]
DMRGB	2.10	2.05	5.36**	4.88**
DMRM	0.60	0.70	2.94	3.34*
DMRN	0.92	0.86	2.63	2.24
DBR	0.35	0.37	1.45	1.03
DYRG	2.10	1.81	4.81**	4.08*
DYRM	4.20*	4.09*	5.16**	4.92**

Notes: See Table 4-1 for definitions of variables. For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form (X/Y)·L(Y), and the right-hand-side Fs are those from a regression where the dependent variable is of the general form L(X), where, in both cases, X is the component of GNP indicated.

F-statistics other than those in parentheses are formed by restricting coefficients of all three lagged shock terms to zero, have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data is available for all lags, and all samples end in 1985): DMR, F(3,31) {1946}; DMRGA and DMRGB, F(3,30) {1947}; DMRM, F(3,17) {1960}; DMRN, F(3,26) {1951}; DBR, F(3,31) {1946}; DYRG and DYRM, F(3,26) {1951}. On F-statistic in parentheses, see note "a" below.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]For L(Y-DINVY) equation using DMRGA shock concept, the coefficient on DMR1 is (marginally) significant and negative at the five percent level. Therefore (see pp. 226-228 of the text) only coefficients on DMRGA2 and DMRGA3 are restricted to equal zero above. F(3,30)=4.88** is the F-statistic where coefficients of all three lagged DMRGAs are restricted to zero.

Y-NRFI-H to rise relative to output, and only a single nominal-GNP-shock concept's significance (at the five percent level) prevents the result found using Barro's shock concept from being robust across the eight shock concepts. Further, there is no evidence that subtracting DINVY out of output creates a category with less of a persistent response to lagged shocks than that revealed by changes-in-inventories themselves. These findings seem difficult to reconcile with the proposition that the inventories-based propagation mechanism plays a major role in generating persistence of real output and unemployment in response to shocks.

All F-statistics in the left-hand-side columns of Table 4-17 were formed by restricting all three lagged demand-shock coefficients to equal zero. Accordingly, an alternative interpretation of these F-statistics is as tests of the null hypothesis that there is no difference in pattern of coefficients between L(Y) and (X/Y)·L(Y). The absence of such a difference in pattern is a fairly robust result when X is set equal to Y-NPDI-H, which suggests that the previous finding that fixed investment categories account for all of the statistically important persistence exhibited by L(Y) is highly robust to changes in shock concept. By contrast, the test leads to a fairly robust rejection of the hypothesis that real output and changes in inventories respond with the same pattern. Thus the results stemming from the pattern tests carried out previously using Barro-type money shocks are robust to change in demand-shock concept, at least in the annual data.

Quarterly Results Using Barro-Type Money Shocks

The relevant findings using quarterly data are presented in Tables 4-11 and 4-14 (for Hypothesis 4) and in Tables 4-18 through 4-20 (for Hypothesis 5). Turning first to an investigation of the first part of Hypothesis 4, Equation 4-11-11 is the quarterly change-in-inventories equation. The pattern of individual shock coefficients supports the annual results and is also consistent with Hypothesis 4, as Barro-type shocks generate moderate short-term positive persistence (DMR1 through DMR4), combined with moderate negative longer-term persistence (DMR10 through DMR13 show t-statistics of 1.8 or larger in absolute value). However, the F-test for the joint significance of all "relevant" lagged shocks fails to reject the null hypothesis of no joint significance. On balance, then, the quarterly DINVY equation cannot be interpreted as supporting Hypothesis 4. Further, as has been seen in previous discussion of Table 4-11, the second part of Hypothesis 4 is contradicted by the data. Both producers' durable expenditures and residential construction exhibit substantial amounts of "strong persistence;" further, nonresidential construction also exhibits as much of a persistent response than do changes in inventories. Hypothesis 4 therefore is not supported by the quarterly findings.

Hypothesis 5 is investigated for quarterly data in Table 4-18. Results support the annual findings, although here the evidence favoring time-to-build over Blinder-Fischer is somewhat weaker. Where the dependent variable is L(Y-NRFI-H) (Equation 4-18-1), no evidence of positive persistence is found, and the F-

TABLE 4-18: QUARTERLY EQUATIONS FOR Y-NRFI-H AND Y-DINVY, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1954:I-1979:III U.S. DATA

Explanatory Variable	(1) L(Y-NRFI-H)	(2) L(Y-DINVY)
Constant	1.30	1.68
	(30.5)	(71.8)
t	0.001	0.001
	(35.4)	(88.0)
LFG	0.07	0.01
	(6.4)	(2.2)
DMR	-0.08	0.05
	(-1.1)	(0.4)
DMR1	-0.17	-0.01
	(-1.7)	(-0.1)
DMR2	-0.38	0.03
	(-3.4)	(0.2)
DMR3	-0.47	0.01
	(-3.9)	(0.1)
DMR4	-0.39	-0.02
	(-3.1)	(-0.2)
DMR5	-0.27	0.08
	(-2.2)	(0.7)
DMR6	-0.22	0.20
	(-1.9)	(1.8)
DMR7	-0.21	0.23
	(-1.8)	(2.1)
DMR8	-0.24	0.24
	(-2.1)	(2.2)
DMR9	-0.06	0.22
	(-0.6)	(2.0)
DMR10	0.02	0.28
	(0.2)	(2.6)
DMR11	0.11	0.28
	(1.1)	(2.6)
DMR12	0.16	0.24
	(1.6)	(2.3)
DMR13	0.06	0.17
	(0.6)	(1.6)
DMR14	0.13	-0.004
	(1.3)	(-0.04)
DMR15	0.12	0.01
	(1.3)	(0.1)
DMR16	0.10	-0.02
	(1.4)	(-0.2)

TABLE 4-18--continued

Explanatory Variable	(1) L(Y-NRFI-H)	(2) L(Y-DINVY)
a1	0.83	0.23
	(7.2)	(2.1)
a2		
a3		
a4	-0.24	
	(-1.6)	
a5		
a6		
a7		
a8		-0.18
		(-1.6)
R^2	0.99	0.99
s	0.003	0.004
Q (6 lags)	4.59	0.12
Q (24 lags)	28.61	17.10
F(16,75)	1.76	2.23*

TABLE 4-18--continued

Explanatory Variable	(3) [(Y-NRFI-H)/Y]·L(Y)	(4) [(Y-DINVY)/Y]·L(Y)
Constant	3.43	5.37
	(15.4)	(32.4)
t	0.01	0.01
	(35.9)	(74.8)
LFG	0.33	0.06
	(6.1)	(1.4)
DMR	-0.41	0.25
	(-1.1)	(0.4)
DMR1	-0.91	-0.11
	(-1.7)	(-0.1)
DMR2	-2.00	0.14
	(-3.4)	(0.2)
DMR3	-2.47	-0.03
	(-3.9)	(-0.04)
DMR4	-2.04	-0.19
	(-3.1)	(-0.3)
DMR5	-1.40	0.51
	(-2.1)	(0.7)
DMR6	-1.15	1.23
	(-1.8)	(1.7)
DMR7	-1.07	1.41
	(-1.7)	(2.0)
DMR8	-1.23	1.47
	(-2.0)	(2.1)
DMR9	-0.27	1.35
	(-0.5)	(1.9)
DMR10	0.17	1.74
	(0.3)	(2.5)
DMR11	0.65	1.79
	(1.2)	(2.6)
DMR12	0.94	1.60
	(1.7)	(2.3)
DMR13	0.36	1.12
	(0.7)	(1.6)
DMR14	0.72	-0.01
	(1.4)	(-0.01)
DMR15	0.70	0.14
	(1.4)	(0.2)
DMR16	0.55	-0.06
	(1.5)	(-0.1)

TABLE 4-18--continued

Explanatory Variable	(3) [(Y-NRFI-H)/Y]·L(Y)	(4) [(Y-DINVY)/Y]·L(Y)
a1	0.82	0.26
	(7.1)	(2.3)
a2		
a3		
a4	-0.25	
	(-1.7)	
a5		
a6		
a7		
a8		
R^2	0.99	0.99
s	0.02	0.03
Q (6 lags)	4.07	0.24
Q (24 lags)	30.60	18.67
F(16,75)	1.84*	1.79

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. Only autocorrelation coefficients with t-values greater than 1.5 are reported. F-statistics are formed by restricting coefficients on the 16 lagged shock terms to zero.

*Estimate is statistically significant at the five percent level.

test for the joint significance of all 16 lagged DMRs fails to reject the null hypothesis of no joint significance. However, a number of individual coefficients are negative and significant, suggesting that, after net fixed investment is removed from GNP, (the log of) what remains decreases in response to, roughly, DMR2 through DMR8.[21] No evidence of a negative and significant effect of shocks on any component of (the log of) Y-NRFI-H is to be found in Table 4-12, so that the inconsistency of Equation 4-18-1 with the findings of Table 4-12 is at least worth noting (since the F-test only just fails to reject the null hypothesis).

Where the dependent variable is L(Y-DINVY) (Equation 4-18-2), results also are favorable to time-to-build and unfavorable to Blinder-Fischer. As was the case for the annual results, a positive and significant effect of older shocks (DMR7 through DMR12) continues after DINVY is subtracted out of GNP, and the F-test for joint significance of all lagged DMRs is rejected at the five percent level. DINVY, therefore, does not account for all of the statistically significant positive persistence exhibited by L(Y). However, Equation 4-18-2 does represent some evidence that the Blinder-Fischer mechanism accounts for some of the persistence of GNP: Short-term persistence disappears once DINVY

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[21] In interpreting these statistically significant individual coefficients, strictly speaking Haraf's point (p. 269, above) still applies: Given the insignificant F, the significance of the individual coefficients is meaningless. However, the failure of the F-test to reject the null hypothesis is fairly marginal (the critical F equals 1.80 versus a reported F of 1.76). Under these circumstances the behavior of individual coefficients may be of some interest, and from time to time below, under similar circumstances, they will enter the discussion.

is subtracted out of GNP. Thus the results confirm the annual findings that, while Blinder-Fischer does not play the sole role in generating GNP-persistence, it does play some role in generating short-term persistence (this in spite of the fact that lagged shocks are jointly insignificant in the DINVY equation).

Equations 4-18-3 and 4-18-4 are the two equations where dependent variables are of the general form $[(Y-X)/Y]\cdot L(Y)$. First, Equation 4-18-3 indicates that subtracting fixed investment out of GNP creates a category which does not rise relative to GNP in response to shocks, while Equation 4-18-4 indicates that subtracting DINVY out of GNP creates a category which does exhibit such a response (roughly for DMR7 through DMR12) when measured by the significance of individual coefficients. The F-test of the null hypothesis that all "relevant" lagged shocks are zero (Fs are presented in Table 4-19) fails to find a joint, statistically significant effect of such DMRs in either equation (although the F for the Y-DINVY equation only just misses significance--critical $F(16,75)=1.80$). Finally, for the F-test for the joint significance of all 16 lagged DMRs--which amounts to a test of the hypothesis that the pattern of persistence exhibited by L(Y) does not differ from that of $(X/Y)\cdot L(Y)$--results mildly support Blinder-Fischer. However, the F-statistic in the Y-DINVY equation again just misses statistical significance (1.79 compared to a critical F of 1.80), while the F-statistic in the Y-NRFI-H equation is only marginally significant (1.83, versus a critical F of 1.80).

Further, the difference between the coefficient pattern on $[(Y-NRFI-H)/Y]\cdot L(Y)$ and that on $L(Y)$ is due to the effect of shocks on NRFI+H exceeding that on the latter--hardly an outcome damaging to time-to-build. In sum, it is concluded that, while results using quarterly data and Barro-type money shocks are more difficult to interpret than the corresponding annual findings, on balance the quarterly results also favor time-to-build over Blinder-Fischer.

Quarterly Results Using Alternative Measures of Nominal-Demand Shocks

Tests of Hypotheses 4 and 5 using quarterly data and alternative nominal-demand-shock concepts yield results similar to those derived using Barro-type shocks. The relevant information is reported in Tables 4-14, 4-19, and 4-20. First, from Table 4-14, results for DINVY are little different when alternative shocks are used as explanatory variables; specifically, only weak evidence is found that shocks cause the GNP-weighted ratio of DINVY to GNP to increase over time. Only one of five money-shock concepts (and that at only the five percent significance level), and only three of seven nominal-demand-shock concepts, are jointly significant in a DINVY equation. The extent of persistence exhibited by DINVY is thus substantially less than that displayed by PDUR and H, but somewhat more than that exhibited by STR. In sum, the weak results exhibited by the DINVY equation when DMR is the shock concept are robust to a variation in shock concept, as is the

finding that DINVY exhibits substantially less persistence than the fixed-investment accounts.

The conclusions previously reached concerning Hypothesis 5 also are robust to a change in shock concept. Where the dependent variable is of the general form L(Y-X), results tend to favor time-to-build over Blinder-Fischer. Where X is NRFI+H, only one of seven nominal-demand-shock concepts rejects the null hypothesis that all "relevant" lagged shock terms are not statistically significant. Where X is DINVY, two of three money-shock concepts, and three of seven nominal-demand-shock concepts, reject this null hypothesis. Similarly, inspection of the two columns of left-hand-side Fs in Table 4-19 reveals only slightly more evidence that lagged shocks cause Y-DINVY to rise relative to Y (two of seven nominal-demand-shock concepts), than the alternative hypothesis that lagged shocks cause Y-NRFI-H to rise relative to Y (one of seven shock concepts). Finally, taking the appropriate Fs from Tables 4-19 and 4-20, the robustness of the results of the "pattern test" can be assessed. Here results do not indicate that either hypothesis outperforms the other. For each case, four of seven shock concepts do not reject the null hypothesis that the pattern of effect of shocks on L(Y) is identical to their effect on $(X/Y)\cdot L(Y)$. In sum, results found using quarterly data and Barro-type shocks weakly favor time-to-build over Blinder-Fischer, and this result is robust to variation in nominal-demand-shock concept.

TABLE 4-19: F-STATISTICS FROM Y-NRFI-H AND Y-DINVY EQUATIONS, TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	Y-NRFI-H		Y-DINVY	
DMR	(1.31)	(1.25)	1.79	2.23*
DMRG	(1.10)	(1.07)	3.52**	3.73**
DMRM	(0.57)	(0.53)	1.26	1.33
DMRN	(3.10)**	(3.18)**	1.33	1.43
DBR	(1.64)	1.64	1.14	1.31
DYRG	(0.65)	(0.68)	(1.34)	(1.29)
DYRM	(0.57)	(0.59)	2.59**	2.31**

Notes: See Table 4-1 for definitions of variables. For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated.

Where all lagged shocks are restricted to zero, for equations using all shock concepts except DMRN F-statistics are F(16,75) and effective sample period is 1954:I-1979:III. For Fs stemming from equations using DMRN as shock concept, F-statistics are F(16,59) and effective sample period is 1958:I-1979:III.

For the definition of "relevant" in the present context, see the text's discussion on pp. 226-228, above. F-statistics in parentheses leave one or more lagged shocks unrestricted; others restrict all lagged shocks to zero. Following is a list of: (a) F-statistic degrees of freedom, and, (b) lagged shocks left unrestricted, for all Fs in parentheses (presented in the general form F(n,d) $(\cdot)$, where within brackets are the numbers corresponding to the DMR lags left unrestricted). Thus the first entry below means that DMR2 through DMR5 plus DMR8 are left unrestricted in the regression equation leading to the F-statistic in the table.

For left-hand-side Fs: for Y-NRFI-H: DMR F(11,75),{2-5,8}; DMRG F(12,75),{1-4}; DMRM F(8,75),{1-8}; DMRN F(8,59),{1-8}; DBR F(15,75),{2}; DMRG F(15,75),{16}; DMRM F(14,75),{15,16}. For Y-DINVY: DYRG F(14,75),{1,2}.

For right-hand-side Fs: for Y-NRFI-H: DMR F(11,75),{2-5,8}; DMRG F(12,75),{1-4}; DMRM F(8,75),{1-8}; DMRN F(8,59),{1-

TABLE 4-19--continued

8); DYRG F(15,75),(16); DYRM F(14,75),(15,16). For Y-DINVY: DYRG F(14,75),(1,2).

F-statistics corresponding to the above which restrict all 16 lagged shocks to zero are presented in Table 4-20.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-20: F-STATISTICS FROM Y-NRFI-H AND Y-DINVY EQUATIONS, TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	Y-NRFI-H		Y-DINVY	
DMR	1.84*	1.76		
DMRG	1.66	1.64		
DMRM	2.84**	2.78**		
DMRN	4.21**	4.22**		
DBR	1.72			
DYRG	0.65	0.68	2.72**	2.78**
DYRM	0.77	0.81		

Notes: See Table 4-1 for definitions of variables. For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated.

F-statistics are formed by restricting coefficients on the 16 lagged shock terms to zero (omitted Fs are reported in Table 4-19). For equations using all shock concepts except DMRN, F-statistics are F(16,75) and effective sample period is 1954:I-1979:III. For Fs stemming from equations using DMRN as shock concept, F-statistics are F(16,59) and effective sample period is 1958:I-1979:III.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

Tests of Time-To-Build Using Independent Survey Data on Production Periods (and Related Tests)

In previous sections, the investigation of the extent of persistence displayed by the various components of GNP has yielded results which are, on the whole, consistent with the time-to-build propagation mechanism. While this is an interesting finding in itself, sharper tests of time-to-build can be carried out, using independent survey data from which estimates can be derived of various construction progress patterns for residential and nonresidential structures and for producers' durable equipment. These progress patterns, previously reported in Chapter 3, are derived from the raw data according to the methods laid out in Appendix B. The uses to which they are put in the present section have previously been described and summarized while developing Hypotheses 6, 7, and 8. Results from carrying out tests of these three hypotheses, for quarterly data over the 1954:I-1979:III sample, using seven nominal-demand-shock concepts, are presented in Table 4-21.

Table 4-21 is set up as follows. Analysis is carried out for the three components of net fixed investment: nonresidential structures, residential structures, and producers' durable expenditures. As is the case for previous tables of this general type, left-hand-side columns list F-statistics corresponding to where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and right-hand-side columns list Fs where the dependent variable is of the form $L(X)$, where X is the component of GNP indicated. Pattern tests are carried out only where previous tests (reported in Table 4-14) indicate that, for a particular dependent

variable, all "relevant" lagged shocks have sufficient explanatory power to be jointly significant (at least at the five percent level). This is appropriate given that the objective of Table 4-21 is to determine whether imposing some time-to-build pattern on a regression equation brings about a statistically significant decline in an equation's explanatory power. If the data cannot reject the hypothesis that all "relevant" lagged shocks are jointly insignificant, then imposing a particular pattern on the coefficients is uninteresting in the present context.

Progress patterns can be imposed on the shock coefficients in the model only if an assumption is made regarding the length of the lag "k" separating the decision to start an investment project (assumed to take place in the period of the shock's occurrence) and the actual date of start. As argued previously (Chapter 3, pp. 128-129), some small amount of persistence by project starts might exist without necessarily contradicting the logic of the Rational Expectations hypothesis. It is difficult to estimate the length of this lag, but it seems likely (Chapter 2, pp. 47-49, above) that it does not exceed two quarters in length for residential structures and producers' durable equipment, and three quarters in length for nonresidential structures. Rather than specify a more exact restriction, each pattern test is carried out for three separate instances, where k equals zero, one, and two quarters, respectively (for STR, for four instances, where k equals zero through three quarters).

Thus, where k=i, the production progress pattern is imposed on the model beginning with the shock occurring in period t-i.

Results for Nonresidential and Residential Construction: Hypothesis 6

The specific nature of the pattern tests imposed on nonresidential and residentiol construction has been discussed in the context of Hypothesis 6 (Chapter 3, pp. 160-161). The first twelve columns of results in Table 4-21 give the findings for nonresidential construction. The first two of these columns, labeled "no shocks," give the results of imposing the restriction that all shocks (including the contemporaneous shock) are restricted to zero, and are supplied for purposes of comparison. The last two of these columns, labeled "L(OCON), k=0," give the results of imposing on STR the null hypothesis that its pattern of coefficients is identical to the pattern of shock coefficients for the (log of the) "starts" concept relevant to STR--which is OCON, construction contracts (measured in square feet of floor space) awarded for commercial and industrial buildings. The issue of "starts" persistence will be examined in detail in a later section. Imposing the "starts" pattern allows the investigation of the interesting alternative hypothesis that the pattern of persistence exhibited by STR is completely explained by the pattern of OCON persistence. Similar usage of "starts"-equation coefficient patterns will also be carried out for H and PDUR. Finally, columns 3 through 10 present the results of imposing on STR the restriction that the pattern of shock coefficients is completely explained by the average quarterly

TABLE 4-21: F-STATISTICS TESTING NULL HYPOTHESIS THAT THE PATTERN OF SHOCK COEFFICIENTS IS IDENTICAL TO THAT OF INDEPENDENT SURVEY DATA GIVING CONSTRUCTION PROGRESS PATTERNS, QUARTERLY DATA, 1954:I-1979:III, SEVEN SHOCK CONCEPTS

Shock Concept	STR (no shocks)		STR (k=0)		STR (k=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
DMR	n.a.	2.12*	n.a.	1.78	n.a.	1.78
DMRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DMRM	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DMRN[a]	2.29**	2.91**	2.15*	2.44**	2.17*	2.62**
DBR	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRM	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.

TABLE 4-21--continued

Shock Concept	STR (k=2)		STR (k=3)		STR (L[OCON], k=0)	
	(7)	(8)	(9)	(10)	(11)	(12)
DMR	n.a.	1.41	n.a.	1.83*	n.a.	1.77
DMRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DMRM	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DMRN[a]	1.84*	2.16*	2.15*	2.68**	2.15*	2.57**
DBR	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRM	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.

TABLE 4-21--continued

Shock Concept	H (no shocks)		H (k=0)		H (k=1)	
	(13)	(14)	(15)	(16)	(17)	(18)
DMR	2.02*	1.91*	2.04*	1.91*	2.11*	1.95*
DMRG	1.85*	n.a.	1.91*	n.a.	1.91*	n.a.
DMRM	2.96**	3.16**	3.01**	3.17**	2.88**	2.99**
DMRN[a]	2.19*	2.08*	2.04*	1.93*	2.23*	2.09*
DBR	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRM	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.

TABLE 4-21--continued

Shock Concept	H (k=2)		H (L[HS], k=0)	
	(19)	(20)	(21)	(22)
DMR	1.66	1.67	1.70	1.48
DMRG	1.55	n.a.	1.51	n.a.
DMRM	2.47**	2.68**	2.24*	2.22*
DMRN[a]	1.99*	1.95*	1.71	1.50
DBR	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.
DYRM	n.a.	n.a.	n.a.	n.a.

TABLE 4-21--continued

Shock Concept	PDUR (no shocks)		PDUR (Rottenberg-Donahoe)		PDUR (k=0)	
	(23)	(24)	(25)	(26)	(27)	(28)
DMR	2.43**	2.98**	2.85**	3.55**	1.04	1.38
DMRG	2.38**	2.56**	2.72**	2.87**	1.08	1.38
DMRM	3.29**	3.21**	3.98**	3.78**	1.06	1.39
DMRN[a]	2.92**	3.89**	3.38**	4.61**	1.68	1.82*
DBR	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRM	n.a	2.23*	n.a	2.19*	n.a	2.02*

TABLE 4-21--continued

Shock Concept	PDUR (k=1)		PDUR (k=2)		PDUR (L[ODUR], k=0)	
	(29)	(30)	(31)	(32)	(33)	(34)
DMR	1.70	2.11*	2.01*	2.63**	2.23*	2.68**
DMRG	1.91*	2.10*	2.17*	2.44**	1.84*	2.01*
DMRM	2.15*	2.16*	2.94**	3.06**	2.65**	2.46**
DMRN[a]	2.36**	2.66**	2.73**	3.41**	2.36**	3.09**
DBR	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRG	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
DYRM	n.a.	2.25*	n.a.	2.24*	n.a.	1.56

Notes: See Table 4-1 for definitions of variables.
n.a.: not applicable (because of the failure of lagged

TABLE 4-21--continued

shocks to exert a significant impact in Table 4-14, imposition of the pattern test does not yield relevant information).

For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated.

The subheading "no shocks" means that the F-test is of the null hypothesis that all shocks (both current and lagged) equal zero. For other F-statistics, coefficient patterns imposed on the unrestricted model in generating the restricted model are taken from the following independent sources: for STR (k=i, where i=0, . . . , 3), Table 3-5, above; for STR (L[OCON], k=0), Table 4-24, below; for H (k=i, where i=0, . . . , 2), Table 3-5, above; for H (L[HS], k=0), Table 4-24, below; for PDUR (Rottenberg-Donahoe), the restriction that all shocks older than four quarters equal zero; for PDUR (k=i, where i=0, . . . , 2), Table 3-10, above; for PDUR (L[ODUR], k=0), Table 4-24, below. The notation "k=i" means that the pattern imposed in generating the restricted model is lagged i periods. See the text for additional discussion.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]Sample for regression equations using DMRN shock concept begins in 1958:I.

progress patterns for nonresidential-construction projects (from Table 3-5, above), for various values of k.

Discussion now can turn to the first part of Hypothesis 6. Results for STR are hampered by the fact that only a weak relationship exists in the quarterly data between STR and nominal-demand shocks. Table 4-14 shows that, of the seven shock concepts, only DMRN generates a jointly significant impact on both $(STR/Y)\cdot L(Y)$ and $L(STR)$, and, of the remaining equations, only DMR where $L(STR)$ is the dependent variable generates a rejection of the null hypothesis. Analysis of the response for these three cases, however, leads to rejection of the null hypothesis that the statistical significance of shocks on STR can be explained by average quarterly progress patterns on nonresidential construction projects. Where DMRN is the shock concept, there is no evidence that time-to-build accounts for the statistical significance of shocks, as lagged shocks continue to exert a significant impact regardless of the value assumed for k. For $L(STR)$ where DMR is the shock concept, the pattern test fails to be rejected for all values of k except where k equals three; however, reference to column 2 indicates that the original F-statistic is only marginally significant to begin with, so that this finding is not of great interest. More important is that there is no evidence that the pattern of construction progress has any more explanatory power than does the behavior of construction project "starts," as there is little to choose between results reported in columns 11 through 12 and any of the

results in columns 3 through 10. Thus the first part of Hypothesis 6 is not supported by the data.

Results for residential construction and the test of the second part of Hypothesis 6 are analogous to the findings for nonresidential construction. The relevant findings are presented in columns 13 through 22 of Table 4-21. There is no substantive evidence that construction progress patterns for H explain the persistence of H, and the data cannot reject the alternative hypothesis that the pattern of persistence exhibited by (the log of) "housing starts" (HS) is identical to the pattern of persistence displayed by H. For the construction progress-pattern tests, the best results are for k equals two; however, it is difficult to believe that residential construction projects have such a long "starts" lag in light of the fact that the bulk of the value of such construction is composed of single-unit structures. Further, the time-to-build pattern with k equal to two is very similar to the "housing starts" pattern with k equal to zero, and it is likely that this rather than time-to-build is the explanation of the relatively superior performance of the "k=2" case among all the construction progress-pattern tests. In sum, the data do not support either portion of Hypothesis 6, for either nonresidential or residential structures.

Results for Producers' Durable Expenditures

Given that the bulk of the explanatory power of fixed investment by lagged shocks is due to the effect of shocks on producers' durable expenditures, the extent to which PDUR can be explained by time-to-build is an important question. Columns 23

through 34 of Table 4-21 are the results for producers' durable expenditures, which are of three basic types. First (columns 25 through 26, Hypothesis 7), the null hypothesis is tested that the unrestricted pattern of response of PDUR to shocks is identical to that of a restricted model where all shocks older than four quarters equal zero. This restriction--labeled "Rottenberg-Donahoe" in Table 4-21--is suggested by the survey data on average production periods within PDUR, due to Rottenberg and Donahoe (1975), Zarnowitz (1973), and Purchasing magazine, and given above in Tables 3-6 through 3-9. The second type of restriction tests some version of the null hypothesis that the pattern of shocks affects PDUR in a way indistinguishable from the average quarterly start-to-completion pattern for nonresidential structures (columns 27 through 32, Hypothesis 8). The process by which this pattern is derived from the raw survey data is discussed in Appendix B, while the resulting pattern is given in Table 3-10, above. The third type of pattern test (columns 33 through 34, labeled "L(ODUR), k=0") is the test of the alternative hypothesis that the pattern of persistence for PDUR is identical to that for "starts" of producers' durable equipment--where "starts" are defined as ODUR, the real value of manufacturers' new orders for durable goods. In addition to the pattern tests of Table 4-21, Tables 4-22 and 4-23 present, for annual data, an exploration of the patterns of persistence exhibited by the components of producers' durable expenditures--an analysis which is complementary to that of Table 4-21.

Results for producers' durable expenditures: Hypothesis 7

In light of the strong and long-lasting persistence exhibited by PDUR, one would expect the data to fairly strongly reject the "Rottenberg-Donahoe" hypothesis, and columns 25 and 26 indicate that this is in fact the case. Relative either to the results of Table 4-14 (where all "relevant" lagged shocks are restricted to zero), or to the results of columns 23 and 24 of Table 4-21 (where all shocks are restricted to zero), no statistically significant increase in explanatory power is observed when shocks from periods t-1 through t-4 are left unrestricted. Results therefore do not support Hypothesis 7, which suggested that average production periods within producers' durable equipment ought to account for the bulk of the explanatory power of lagged shocks.

Results for producers' durable expenditures: Hypothesis 8

Results for Hypothesis 8, in contrast to those for Hypothesis 7, are consistent with the hypothesis. The crucial result is reported in columns 27 and 28 of Table 4-21, where the null hypothesis is tested, for k equal to zero, that the pattern of effect of shocks on PDUR is indistinguishable from the average quarterly start-to-completion pattern for nonresidential structures. Columns 27 and 28 indicate broad support for the null hypothesis: In all cases, dramatic declines in F-statistics occur relative to where all shocks are restricted to zero (columns 23 through 24). No shock concept continues to elicit a statistically significant amount of persistence from $(PDUR/Y)\cdot L(Y)$, and, while two of five shocks elicit such a

response from L(PDUR), the significance levels increase from one to five percent. Columns 29 through 32 impose the start-to-completion restriction where k equals one and two, respectively. Results indicate some support for the null hypothesis where k equals one but none where k equals two. These additional findings also are consistent with Hypothesis 8: No substantial lag should separate the completion quarter for a nonresidential-structures construction project and the quarter in which expenditures are incurred on the equipment to fill that structure (expenditures on producer durables are mainly counted in the National Income and Product accounts in the quarter of shipment).

Finally, columns 33 through 34 present the results for the alternative hypothesis that the response of PDUR to shocks is identical to the response of (the log of) durable-goods "starts", where such starts are measured as orders for durable goods by manufacturing firms. Results strongly reject the null hypothesis, as no discernible difference can be observed between these results and those where all shocks are restricted to equal zero. Producers' durable expenditures clearly respond to nominal-demand shocks with a pattern of persistence different from that of durable-goods "starts."

In sum, results of the pattern tests for producers' durable expenditures, taken alone, strongly suggest that the considerable persistence exhibited by this output category is attributable to time-to-build effects stemming from the complementary relationship existing between nonresidential structures and producers' durable equipment. However, this raises the question

of why an analogous relationship does not exist between expenditures on nonresidential structures and quarterly construction progress patterns for nonresidential structures. The behavior of nonresidential structures continues, as in previous sections, to be a problem for the time-to-build hypothesis. Further, the failure of the time-to-build explanation of the persistence of residential structures also is notable in the present context. The contradiction between the results for construction and those for producers' durable equipment thus undermines the support for Hypothesis 8 somewhat.

Analysis of persistence of the components of producers' durable expenditures

It has just been seen that, while the pattern tests for producers' durable expenditures generate results which are favorable to the time-to-build propagation mechanism, other pattern tests for nonresidential and residential construction are in conflict with this result. The conflict between nonresidential structures and producer durables is of particular importance: Since the source of the pattern test for producer durables is survey data on nonresidential structures, it is unclear why one test should lead to results consistent with time-to-build and the other to findings in conflict with the hypothesis (this is still true even though different surveys are used to restrict the two output categories). Under these circumstances it is reasonable to delve more deeply into the relationship between producers' duroble expenditures and nominal-demand shocks. Accordingly, Tables 4-22 and 4-23 investigate the persistence of the various disaggregated components of PDUR,

exploiting National Income and Product Accounts data which supply a detailed disaggregation by type of product of annual producers' durable expenditures.

Table 4-22 presents the results for PDUR and a number of its components using Barro-type money shocks as explanatory variables. Analysis is for the 1947-85 period, where dependent variables are of the general form $(X/Y)\cdot L(Y)$, X equalling either PDUR or some component of PDUR. Results are consistent with the time-to-build hypothesis. Equation 4-22-1 presents the results for PDUR for purposes of comparison, while the response to shocks of the primary components of producers' durable equipment is presented in Equations 4-22-2, 4-22-4, 4-22-5, and 4-22-6. Of these components, time-to-build predicts that "industrial equipment" (PDINDL) will show the most persistence and this is the case when the measurement criterion is the number of lagged shocks which are significant: Alone among the major components, PDINDL shows a persistent response both to DMR1 and DMR2. Both "transportation and related equipment" (PDTRANS) and "other equipment" (PDELSE) show a single year's persistence. The F-statistic testing for the joint significance of the three lagged DMRs rejects the null hypothesis at the one percent level for PDTRANS and PDELSE and at the five percent level for PDINDL (this last rejection being somewhat weaker than would be predicted). The remaining component, "information processing and related equipment" (PDINF), fails to show an explanatory role for Barro-type shocks, a result presumably due to the rise of the computer

TABLE 4-22: ANNUAL EQUATIONS FOR THE MAJOR COMPONENTS OF PRODUCERS' DURABLE EXPENDITURES, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1947-85 U.S. DATA, "(X/Y)·L(Y)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF PDUR)

Explanatory Variable	(1) PDUR	(2) PDINF	(3) PDINFA	(4) PDINDL	(5) PDELSE
Constant	0.68	-0.20	-0.09	0.28	0.34
	(4.5)	(-2.0)	(-2.8)	(4.5)	(3.1)
t	0.001	0.005	0.003	0.001	-0.01
	(1.7)	(6.6)	(9.9)	(2.6)	(-6.0)
LFG	-0.12	-0.01	-0.01	-0.04	-0.03
	(-3.6)	(-0.7)	(-1.6)	(-2.6)	(-1.2)
DMR	1.13	0.01	0.02	0.15	0.44
	(3.1)	(0.04)	(0.4)	(1.2)	(2.6)
DMR1	2.00	0.05	0.11	0.54	0.82
	(4.7)	(0.2)	(1.8)	(3.3)	(3.6)
DMR2	1.03	0.21	0.14	0.37	0.31
	(2.4)	(0.9)	(2.1)	(2.2)	(1.4)
DMR3	0.23	0.04	0.07	0.17	0.08
	(0.6)	(0.2)	(1.3)	(1.3)	(0.5)
a1	0.57	0.74	0.93	0.80	1.07
	(3.3)	(4.0)	(5.1)	(4.7)	(6.1)
a2	-0.30	-0.001	-0.10	-0.34	-0.26
	(-1.7)	(-0.004)	(-0.55)	(-2.0)	(-1.5)
R^2	0.74	0.95	0.99	0.73	0.97
s	0.03	0.02	0.004	0.01	0.01
Q (6 lags)	7.33	30.72**	9.77	10.68	2.91
F(3,30)	7.52**	0.44	1.52	3.75*	6.26**

TABLE 4-22--continued

Explanatory Variable	(6) PDTRANS	(7) PDTRBIG	(8) PDTRSMALL	(9) PDTRCKS	(10) PDAUTOS
Constant	0.25	0.07	0.16	0.09	0.06
	(4.3)	(2.1)	(3.3)	(2.6)	(2.3)
t	0.002	0.00004	0.001	0.001	0.0004
	(4.8)	(0.2)	(5.5)	(5.0)	(2.8)
LFG	-0.04	-0.007	-0.03	-0.02	-0.01
	(-3.4)	(-0.9)	(-3.1)	(-2.6)	(-1.9)
DMR	0.44	0.13	0.31	0.20	0.10
	(3.1)	(1.8)	(2.6)	(2.7)	(1.4)
DMR1	0.65	0.28	0.38	0.34	0.02
	(4.0)	(3.2)	(2.8)	(3.6)	(0.3)
DMR2	0.28	0.25	0.06	0.08	-0.04
	(1.7)	(2.7)	(0.5)	(0.9)	(-0.6)
DMR3	0.06	-0.01	0.12	0.03	0.08
	(0.4)	(-0.1)	(1.0)	(0.4)	(1.1)
a1	0.45	0.66	0.62	0.78	0.46
	(2.5)	(3.7)	(3.7)	(4.5)	(2.6)
a2	-0.14	-0.19	-0.37	-0.34	-0.21
	(-0.8)	(-1.0)	(-2.2)	(-2.0)	(-1.2)
R^2	0.75	0.63	0.76	0.82	0.46
s	0.01	0.01	0.01	0.01	0.01
Q (6 lags)	7.96	9.18	6.76	5.13	6.42
F(3,30)	5.44**	4.50*	3.82*	5.78**	0.94

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. F-statistics are formed by restricting coefficients on the three lagged shock terms to zero.

TABLE 4-22--continued

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-23: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, MAJOR COMPONENTS OF PRODUCERS' DURABLE EXPENDITURES, ANNUAL U.S. DATA, VARIOUS PERIODS FROM 1947-85, EIGHT SHOCK CONCEPTS

Shock Concept	(1) PDUR	(2) PDINF	(3) PDINFA	(4) PDINDL	(5) PDELSE
DMR	7.52**	0.44	1.52	3.75*	6.26**
DMRGA	3.05*	1.58	2.78	1.56	2.63
DMRGB	8.16**	0.43	2.00	6.73**	8.23**
DMRM	2.31	2.60	0.08	6.60**	2.30
DMRN	4.36	0.34	2.09	4.56*	3.40*
DBR	8.65**	0.18	0.70	3.77*	1.96
DYRG	1.74	3.67*	0.49	1.31	0.27
DYRM	3.62*	2.07	0.93	1.65	1.61

TABLE 4-23--continued

Shock Concept	(6) PDTRANS	(7) PDTRBIG	(8) PDTRSMALL	(9) PDTRCKS	(10) PDAUTOS
DMR	5.44**	4.50*	3.82*	5.78**	0.94
DMRGA	2.38	1.86	2.71	3.62*	1.83
DMRGB	7.67**	3.31*	4.68**	8.06**	0.29
DMRM	2.37	3.17	1.41	1.22	2.84
DMRN	4.29*	2.76	3.19*	3.95*	1.77[a]
DBR	6.92**	4.28*	2.04	4.92**	0.15
DYRG	1.18	1.32	1.37	2.09	2.24
DYRM	2.35	3.64*	2.50	4.03*	1.72

TABLE 4-23--continued

Notes: See Table 4-1 for definitions of variables. All F-statistics are formed from a regression where the dependent variable is of the general form (X/Y)·L(Y), where X is either PDUR or a component of PDUR. F-statistics are formed by restricting coefficients on the three lagged shock terms to zero (except for where indicated in note "a," below). F-statistics have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data is available for all lags, and all samples end in 1985): DMR, F(3,30) {1947}; DMRGA and DMRGB, F(3,30) {1947}; DMRM, F(3,17) {1960}; DMRN, F(3,26) {1951}; DBR, F(3,30) {1947}; DYRG and DYRM, F(3,26) {1951}.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]Regression equation contains negative and significant coefficient on DMRN2. If this shock is left unrestricted, the resulting F is F(2,26)=0.17.

industry in recent years.[22] Some support for this conclusion is provided by Equation 4-22-3, which investigates the persistence of PDINFA, defined as that part of PDINF other than "office, computing, and accounting machinery." The equation shows some minimal evidence of persistence, with DMR2 being statistically significant and DMR1 just missing significance, although, since the F-test fails to reject the null hypothesis of no joint significance of lagged DMRs, these findings should be heavily discounted.

Equations 4-22-7 through 4-22-10 present a disaggregation of PDTRANS, the "transportation and related equipment" category. PDTRANS can be broken down into "aircraft, ships, boats, and railroad equipment" (PDTRBIG) and "trucks, buses, truck trailers, and autos" (PDTRSMALL). Time-to-build predicts that expenditures on larger, more complex equipment should show more persistence than expenditures on smaller equipment. This prediction is consistent with the data, since both F-statistics are significant at the five percent level, and the lag length of significant DMRs is longer for PDTRBIG (DMR2 and DMR1 are significant, as opposed to only DMR1 for PDTRSMALL). Next, a disaggregation of PDTRSMALL was carried out in order to explore the source of the persistence exhibited by this component: The category was broken into "trucks, buses, and truck trailers" (PDTRCKS) and "autos" (PDAUTOS). Time-to-build predicts the absence of persistence in PDAUTOS, since neither lengthy production periods nor a

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[22]PDINF rises from six percent of PDUR in 1947 to 11 percent in 1967 and to 33 percent in 1984. It becomes the largest major component of PDUR in 1982.

complementary relationship between the category and nonresidential construction can here be invoked. By contrast, persistence in PDTRCKS might be consistent with time-to-build, since such products would tend to be ordered by companies so that they would arrive at about the time of completion of, say, a distribution center (there is also the outside chance of there being a lengthy average production period for this category). Again, results are consistent with time-to-build: Substantial persistence is observed in PDTRCKS, and none in PDAUTOS. Thus, results stemming from the disaggregation of producers' durable equipment using Barro-type money shocks support the time-to-build hypothesis.

Table 4-23 explores the robustness of the results from Table 4-22 to a variation in nominal-demand-shock concept. Results may be presented without further discussion, as they support the findings of Table 4-22 in every respect. Thus, it is concluded that the components of producers' durable expenditures respond to nominal-demand shocks in a manner consistent with the time-to-build hypothesis. This in turn lends support to the previous finding that persistence of producers' durable expenditures can be fully explained by the survey data on the number of months from start to completion of nonresidential construction projects.

Analysis of Decisions to Start Multiperiod Investment Projects: Hypothesis 9

The time-to-build propagation mechanism predicts that, assuming a single-period information lag, decisions by firms to start multiperiod investment projects should not show

persistence once allowance is made for the possibility that some short-term persistence of such "starts" might be observed due to institutional factors such as those previously discussed (Chapter 3, pp. 128-129). The investigation of this hypothesis (Hypothesis 9) is carried out in the present section. The persistence of ten "starts" concepts is investigated, for quarterly U.S. data, using both Barro-type money shocks and six other nominal-demand-shock concepts. Results are reported in Tables 4-24 through 4-26.

Definitions of "Starts" Concepts

Table 4-24 reports quarterly regression equations for the ten "starts" concepts, derived using Barro-type money shocks as explanatory variables (except where otherwise indicated, the effective sample period is 1954:I-1979:III). Equations 4-24-1 and 4-24-2 are two equations where the dependent variable is (the log of) orders for some portion of the goods making up producers' durable equipment. The dependent-variable concept in Equation 4-24-1 is the real value of new orders received by manufacturers in the durable-goods industries (ODUR). Many (but not all) of such orders ought to be for goods which are components of producers' durable equipment. The Handbook of Cyclical Indicators (hereafter referred to as HCI) describes new orders as "intents to buy for immediate or future delivery. Only orders supported by binding legal documents, such as signed contracts, letters of intent, or letters of award, are included" (U.S. Department of Commerce, 1984, p. 15). The value of cancellations of existing orders is excluded from the series. The series themselves are

not collected directly, but are derived from monthly Census Bureau survey data on shipments and unfilled orders.

A problem with Equation 4-24-1 is that its dependent variable includes the value of orders received by defense-goods industries. Equation 4-24-2 reports results where the dependent variable is (the log of) a subgroup of ODUR; specifically, orders received by durable-goods manufacturers in nondefense capital-goods industries (ONDK). The category consists of the nondefense portions of nonelectrical and electrical machinery, transportation equipment, and fabricated metals. Prior to 1968, all machinery and equipment industries are included in the series, so that data before 1968 are not directly comparable with those after this date. As a means of compensating for this discrepancy, a dummy variable was introduced into Equation 4-24-2, set equal to zero for all quarters prior to 1968 and set equal to one for the remainder of the sample.

Equation 4-24-3 analyzes the response to shocks of (the log of) newly-approved capital appropriations of the 1000 largest manufacturing corporations (ranked by total firm assets) (CAM). According to HCI, "a 'capital oppropriation' constitutes authority to incur obligations for new plant and equipment that has been granted by the board of directors or president of the company" (U.S. Department of Commerce, 1984, p. 22). Such appropriations are defined to include construction of new buildings and plants, improvements of existing buildings and plants, and equipment. Further, "wherever possible, appropriations for land, maintenance and repair, used equipment

and buildings, construction and equipment outside the United States, acquisition of existing companies, and leases for land or mineral rights are excluded" (U.S. Department of Commerce, 1984, p. 22). CAM thus tracks intentions to start projects which are counted as nonresidential fixed investment in the GNP accounts. HCI calls CAM "a barometer of business planning and expectations" (U.S. Department of Commerce, 1984, p. 23). Thus, for this concept it is particularly clear that, if the persistence of shocks is due to time-to-build, then no "substantial" persistence should be observed--probably none at all, and certainly none should be observed after quarter t-2.

Equation 4-24-4 has as its dependent variable the {log of the} sole "starts" concept investigated which measures intentions to start only nonresidential construction projects: OCON, construction contracts awarded for commercial and industrial buildings. The series measures the square feet of floor space "specified in new contracts for work about to get underway on commercial buildings (banks, offices and lofts, stores, warehouses, garages, and service stations) and manufacturing buildings (for processing or mechanical use)" (U.S. Department of Commerce, 1984, p. 22). Since OCON measures construction "about to get underway," it captures changes somewhat further down in the chain of decisions leading to capital formation than some of the other "starts" concepts here investigated--such as CAM, for example, which measures only the intention to award contracts for capital-formation purposes. Again, however, it is clear that, if

time-to-build is the key to explaining the persistence of GNP, then such a "starts" concept should not exhibit "substantial" persistence.

Equations 4-24-5 and 4-24-6 are two equations in which the dependent variable is (the log of) some measure related to starts of residential construction projects. Equation 4-24-5 evaluates the persistence of (the log of) new private housing units started (HS). HCI defines a housing "unit" as "a single room or group of rooms intended for occupancy as separate living quarters by a family, by a group of unrelated persons living together, or by a person living alone" (U.S. Department of Commerce, 1984, p. 24). Various forms of rooming houses and all "transient accommodations" are excluded. HCI defines a "housing start" as "construction begun on a new building that is intended primarily as a housekeeping residential building designed for nontransient occupancy. All housing units in a multifamily building are counted as started when excavation of the foundation begins" (U.S. Department of Commerce, 1984, p. 24). Mobile homes and conversions of residential or nonresidential units to provide additional units are excluded from the definition.

Equation 4-24-6 is a second equation relating to residential construction projects, measuring the persistence of (the log of) decisions to start such projects: an index of new private housing units authorized by local building permits (1967=100) (OH). According to HCI, the data forming the index "relate to the issuance of permits, not to the start of construction, which frequently occurs several months later" (U.S. Department of

Commerce, 1984, p. 24). OH thus resembles several previous "starts" concepts in that the series captures intentions to start future projects rather than actual starts of projects. Accordingly, time-to-build predicts that such a series should be sensitive to a contemporaneous nominal-GNP shock, and insensitive to lagged shocks.

Equations 4-24-7 and 4-24-8 investigate the persistence of two series reflecting manufacturers' evaluations of the adequacy of their plant and equipment facilities. From December 1963 through September 1983 the Commerce department, in their quarterly plant and equipment survey, collected data on manufacturers' evaluation of their plant and equipment facilities (this study uses a subset of the sample, 1963:IV-1979:III). Manufacturers were asked to evaluate their existing facilities as fitting into one of the following three categories: "more facilities needed," "facilities about adequate," or "existing facilities exceeds needs." The persistence of the first of these three ("MORE," in Equation 4-24-7) and of the last ("LESS," in Equation 4-24-8) are investigated in the present study. These series are of particular interest in that they would be expected to capture changes occurring at the earliest stage of the capitol-acquisition process. Thus, there is no prospect of even short-term persistence in these series being consistent with the time-to-build hypothesis, since the arguments about lags due to technological constraints, applicable in the case of "starts" proper, are inapplicable where the dependent variable is the change in firms' assessments of the adequacy of their facilities.

Last, Equations 4-24-9 and 4-24-10 are two equations where the dependent variable is planned expenditures for new plant and equipment by all industries, expressed as a percentage of actual expenditures. The data have been collected since 1955 by the Department of Commerce in their quarterly plant and equipment survey (1955 is also the start of the sample used in this study). Planned expenditures are for one quarter ahead of the survey quarter in Equation 4-24-9 (PLANONE), and for two quarters ahead in Equation 4-24-10 (PLANTWO). These analyses are carried out to gauge the extent to which nominal-demand shocks cause planned expenditures for a future period to diverge from the actual expenditures for that period. In the case of a positive shock, cancellation and/or postponement of projects might be an empirically important option carried out by firms in the period(s) following a positive nominal-demand shock. In the case of a negative shock, a speeding-up of the capital-accumulation process (relative to what previously had been planned) might be observed in the period(s) following such a shock. If such is the case, then there ought to be a positive and significant coefficient on DMR1 in Equation 4-24-9, and an analogous coefficient on DMR2 in Equation 4-24-10. Given that the magnitude and effects of the cancellation of projects is an important unaddressed question in the time-to-build propagation mechanism, empirical evidence which relates to this issue is of interest.

Results Using Barro-Type Money Shocks

Having presented the various "starts" concepts, discussion can turn to the results derived using Barro-type shocks, presented in Table 4-24 and in the first rows of Tables 4-25 and 4-26. Results are not fully consistent, but are best summarized as not supporting Hypothesis 9. There is a general (though not universal) tendency for "starts" to show substantial persistence in excess of that which can be explained in terms of a lag between the decision to undertake a project and the actual date of start. This tends to be the case when the statistical significance of individual DMR coefficients is the criterion of evaluation, or when the criterion is an F-statistic formed by restricting all "relevant" lagged shocks to zero, or when the criterion adopted is an F-stotistic (reported in Table 4-25) formed by restricting all "relevant" lagged DMRs older than t-k to equal zero (where k is the number of quarters after the date of the shock over which persistence can be observed without clearly violating the time-to-build hypothesis). Thus results for "starts" using Barro-type shocks raise questions about the ability of time-to-build--at least in its "pure" form--to account for the persistent response to such shocks of GNP and its components.

Equations 4-24-1 and 4-24-2 analyze the persistence of manufacturers' decisions to order--that is, to bring about the start of--producers' durable equipment. Evidence formed where the dependent-variable concept includes all durable-goods industries (Equation 4-24-1) is inconclusive, as, while DMR

TABLE 4-24: QUARTERLY EQUATIONS FOR TEN "STARTS" CONCEPTS, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, U.S. DATA, VARIOUS PERIODS OVER 1954:I-1979:III

Explanatory Variable	(1) L(ODUR)	(2) L(ONDK)	(3) L(CAM)	(4) L(OCON)	(5) L(HS)	(6) L(OH)
Dummy[a]		0.18				
		(3.2)				
Constant	5.41	3.95	0.58	3.82	7.25	5.79
	(7.2)	(5.0)	(0.3)	(4.2)	(6.3)	(4.7)
t	0.008	0.01	0.01	0.008	0.002	0.002
	(12.1)	(11.5)	(6.9)	(12.1)	(2.4)	(1.5)
LFG	-0.21	-0.30	0.30	0.15	-0.36	-0.28
	(-1.2)	(-1.5)	(0.7)	(0.7)	(-1.3)	(-1.0)
DMR	2.59	4.05	-0.33	4.14	5.99	7.23
	(2.5)	(3.2)	(-0.1)	(2.4)	(3.6)	(4.3)
DMR1	4.99	5.94	3.14	7.45	9.52	11.16
	(3.2)	(3.5)	(0.9)	(3.5)	(3.8)	(4.3)
DMR2	6.77	9.37	9.59	10.55	10.47	11.27
	(3.7)	(4.6)	(2.4)	(4.4)	(3.6)	(3.7)
DMR3	8.16	9.89	10.36	10.91	7.24	7.06
	(3.9)	(4.3)	(2.4)	(4.2)	(2.3)	(2.1)
DMR4	7.72	9.66	9.38	14.74	4.20	3.32
	(3.6)	(4.0)	(2.0)	(5.6)	(1.3)	(1.0)
DMR5	5.66	9.53	7.55	11.69	1.32	2.29
	(2.6)	(3.9)	(1.6)	(4.4)	(0.4)	(0.6)
DMR6	4.43	6.48	8.86	11.79	2.13	3.31
	(2.0)	(2.9)	(1.9)	(4.7)	(0.7)	(1.0)
DMR7	4.53	6.74	5.85	11.18	1.37	1.49
	(2.1)	(3.0)	(1.3)	(4.5)	(0.4)	(0.4)
DMR8	3.44	6.06	2.52	6.23	0.28	1.40
	(1.6)	(2.7)	(0.5)	(2.6)	(0.1)	(0.4)
DMR9	1.95	3.44	4.36	4.46	0.12	-0.24
	(1.0)	(1.6)	(1.0)	(1.9)	(0.04)	(-0.1)
DMR10	0.15	2.19	2.88	1.50	-2.97	-2.40
	(0.1)	(1.0)	(0.7)	(0.7)	(-1.1)	(-0.8)
DMR11	-2.64	-2.07	-0.79	-2.83	-2.81	-0.78
	(-1.4)	(-1.0)	(-0.2)	(-1.3)	(-1.0)	(-0.3)
DMR12	-1.80	-1.78	-2.96	-1.91	0.32	-0.32
	(-1.0)	(-0.9)	(-0.8)	(-0.8)	(0.1)	(-0.1)
DMR13	-1.89	-2.15	-0.93	-4.44	0.49	1.09
	(-1.1)	(-1.1)	(-0.2)	(-2.0)	(0.2)	(0.4)
DMR14	-0.24	-0.56	-1.97	-5.10	1.98	1.80
	(-0.1)	(-0.3)	(-0.6)	(-2.4)	(0.8)	(0.7)
DMR15	-0.35	0.94	-2.01	-3.90	-0.31	-0.30
	(-0.2)	(0.6)	(-0.6)	(-2.0)	(-0.13)	(-0.1)
DMR16	-0.76	-1.59	-0.42	-4.52	0.62	1.14
	(-0.8)	(-1.3)	(-0.2)	(-2.6)	(0.4)	(0.7)

TABLE 4-24--continued

Explanatory Variable	(1) L(ODUR)	(2) L(ONDK)	(3) L(CAM)	(4) L(OCON)	(5) L(HS)	(6) L(OH)
a1	1.07	0.77	0.96	0.58	0.98	1.07
	(9.3)	(6.7)	(8.5)	(5.0)	(8.5)	(9.2)
a2		0.22		0.24		
		(1.5)		(1.8)		
a3						
a4		-0.34		-0.25		
		(-2.3)		(-1.8)		
a5			0.23			
			(1.5)			
a6						
a7	-0.25					
	(-1.5)					
a8						
R^2	0.99	0.99	0.96	0.97	0.92	0.93
s	0.04	0.05	0.09	0.07	0.07	0.07
Q (6 lags)	2.50	5.59	3.59	0.85	5.11	9.31
Q (24 lags)	10.14	23.66	21.81	17.95	22.13	25.64
$F(n,d)^b$	1.78	2.18*	1.17	3.50**	2.03*	2.35**

TABLE 4-24--continued

Explanatory Variable	(7) "MORE"	(8) "LESS"	(9) PLANONE	(10) PLANTWO
Constant	26.33	8.29	106.04	68.87
	(0.4)	(0.6)	(6.4)	(3.1)
t	-0.18	0.03	-0.01	-0.04
	(-2.1)	(2.0)	(-1.1)	(-2.2)
LFG	8.19	-1.19	-1.06	8.47
	(0.6)	(-0.3)	(-0.3)	(1.6)
DMR	-68.07	-135.15	59.98	-22.88
	(-0.8)	(-3.2)	(1.2)	(-0.4)
DMR1	-57.03	-96.37	11.83	-58.61
	(-0.4)	(-1.7)	(0.2)	(-0.9)
DMR2	5.00	-166.43	2.49	-103.10
	(0.03)	(-2.7)	(0.04)	(-1.4)
DMR3	67.72	-159.03	-29.29	-136.16
	(0.4)	(-2.5)	(-0.5)	(-1.9)
DMR4	112.42	-228.24	-24.66	-114.24
	(0.6)	(-3.7)	(-0.4)	(-1.6)
DMR5	153.23	-213.33	-7.79	-75.56
	(0.8)	(-3.5)	(-0.1)	(-1.1)
DMR6	360.49	-184.15	25.72	3.02
	(2.0)	(-3.1)	(0.5)	(0.04)
DMR7	332.36	-153.95	-4.77	41.59
	(1.9)	(-2.6)	(-0.1)	(0.6)
DMR8	284.13	-58.97	7.53	21.76
	(1.6)	(-1.0)	(0.1)	(0.3)
DMR9	144.04	4.48	-39.40	88.42
	(0.9)	(0.1)	(-0.8)	(1.3)
DMR10	173.48	129.25	66.60	190.80
	(1.1)	(2.4)	(1.3)	(2.8)
DMR11	98.36	209.42	-2.73	150.54
	(0.6)	(4.1)	(-0.1)	(2.2)
DMR12	62.80	212.43	31.34	102.55
	(0.4)	(4.3)	(0.6)	(1.5)
DMR13	83.45	128.40	38.29	116.26
	(0.5)	(2.6)	(0.8)	(1.7)
DMR14	-65.86	57.77	70.52	58.42
	(-0.5)	(1.1)	(1.4)	(0.9)
DMR15	-62.60	14.42	63.50	170.28
	(-0.5)	(0.3)	(1.3)	(2.6)
DMR16	-53.88	14.90	53.41	41.93
	(-0.7)	(0.4)	(1.1)	(0.7)

TABLE 4-24--continued

Explanatory Variable	(7) "MORE"	(8) "LESS"	(9) PLANONE	(10) PLANTWO
a1	1.15	0.68	0.29	0.40
	(6.9)	(4.1)	(2.4)	(3.4)
a2				
a3				
a4				
a5				-0.21
				(-1.6)
a6				0.21
				(1.6)
a7				
a8				
R^2	0.93	0.90	0.29	0.55
s	2.61	1.27	1.97	2.45
Q (6 lags)	3.69	8.38	0.12	0.31
Q (24 lags)	18.63	31.95	21.20	15.54
$F(n,d)^b$	1.34	2.12*	0.54	0.75

Notes: See Table 4-1 (and pp. 333-338 of the text) for definitions of variables. t-statistics for a given coefficient are in parentheses immediately below the coefficient. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. Only values for ai with t-statistics of 1.5 or greater are reported.

Sample periods begin in 1954:I for all equations except "MORE" and "LESS," for which samples begin in 1963:IV, and PLAN1 and PLAN2, for which samples begin in 1955:I. All samples end in 1979:III.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-24--continued

[a]See text (p. 334) for explanation of dummy variable.

[b]F-statistic is formed by restricting to zero the coefficients of all "relevant" logged DMRs, where "relevant" is defined as specified on pp. 226-228, above. F(16,75) holds for all equations except the following (for which F-test degrees of freedom follow): LONDK, F(16,74); LOCON, F(12,75); "MORE," F(16,36); "LESS," F(13,36); PLANONE, F(16,71); PLANTWO, F(15,71). The corresponding F-stotistics where all lagged DMRs are restricted to zero are reported in Table 4-26.

through DMR7 all are statistically significant as measured by t-statistics on individual coefficients, the F-test for the joint significance of all lagged shocks fails to reject the null hypothesis. The individual coefficients are here of some interest because the F-statistic just misses being significant (five percent critical F(16,75)=1.80 versus an observed F of 1.78), and because, as Table 4-25 indicates, the F-test for the joint significance of DMR3 through DMR16 rejects the null hypothesis at the five percent level. Further, when the dependent-variable concept is the nondefense portion of ODUR (ONDK, in Equation 4-24-2), results more strongly indicate a persistent response to Barro-type shocks: Here the lag of statistically significant coefficients is longer (DMR through DMR8), and both F-tests reject the null hypothesis, at the one percent level where k equals two. Thus the weak results in Equation 4-24-1 can be attributed to the inclusion in ODUR of defense goods, a category not particularly sensitive to cyclical factors.

Whether orders for producers' durable equipment exhibit too much persistence to be consistent with time-to-build depends on which of the two possible explanations presented previously for producer-durable persistence is correct. If production periods within producers' durable equipment account for the persistence of this category (and previously it has been argued that this is unlikely), then a persistent response by orders of such goods of up to two years is difficult to reconcile with the time-to-build hypothesis. However, if one is willing to invoke a complementary

relationship between nonresidential construction and producers' durable equipment, such persistence need not necessarily conflict with time-to-build. The strength of the evidence for such an interpretation, as well as the difficulties with it--the chief difficulty being the weak response to shocks of nonresidential structures in the quarterly data--have already been discussed above (pp. 322-324). Thus persistence in producer-durable "starts" may be harmonized with time-to-build. This is not the case, however, for "starts" concepts which primarily relate to the other components of fixed investment.

Equation 4-24-3 is the Barro-type equation where the dependent-variable concept is CAM, capital appropriations by the 1000 largest manufacturing firms. Overall, results here are consistent with Hypothesis 9. While DMR2 through DMR4 are statistically significant, the F-tests for joint significance both where k equals zero and two fail to reject the null hypothesis of no joint significance. One might expect to see a statistically significant coefficient on DMR and/or DMR1 on time-to-build premises, but without further information about the dynamics of the appropriations process it is difficult to reach definite conclusions.

To this point results are easily interpreted in terms consistent with time-to-build, but remaining results are not so easily reconciled with the hypothesis. Equation 4-24-4 is the equation where the dependent-variable concept is OCON, construction contracts awarded for nonresidential-construction projects (measured in square feet of floor space). Since this

concept is composed of projects about to begin, one could reconcile two- or even three-quarter persistence for OCON with time-to-build. However, in Equation 4-24-4 two years of persistence follow a money shock, and the F-statistics are significant at the one percent level for both k equals zero and two. Starts of nonresidential construction projects thus respond with far too much persistence to be consistent with time-to-build.

Analogous conclusions are to be drawn from Equations 4-24-5 and 4-24-6, for which the dependent-variable concepts are, respectively, housing starts and housing "orders"--that is, residential building permits. Here time-to-build predicts little or no persistence, certainly not more than one quarter's. Yet DMR through DMR3 are statistically significant in both equations, and the F-tests for joint significance where k equals zero and one reject the null hypothesis in every case. In fact, the patterns of persistence exhibited by L(HS) and L(OH) are similar to that displayed by residential construction, and, as has been seen previously (p. 320, above), it is impossible to reject the null hypothesis that the persistence of housing starts accounts for the persistence of residential construction. Thus "starts" of residential projects also exhibit too much persistence to be consistent with time-to-build.

Next, Equations 4-24-7 and 4-24-8 are two equations where the persistence of manufacturers' evaluation of their plant and equipment capacity is analyzed. These series are of special interest (even though the sample is somewhat smaller) because

there is no plausible argument stemming from institutional rigidities in the capital-construction process which can account for any persistence being exhibited by these dependent variables. One would expect a response to DMR, but assuming a one-period information lag, there is nothing in the dynamics of capital accumulation to suggest persistence. Viewed in this light, Equation 4-24-8 constitutes evidence against Hypothesis 9 and time-to-build. Barro-type shocks have a strong, persistent, and inverse impact on the percentage of manufacturers reporting that their existing capacity "exceeds needs." Not only DMR but also DMR2 through DMR7 have negative and statistically significant coefficients (and, in addition, DMR1 narrowly misses being significant). The F-test for the joint significance of all "relevant" lagged shocks rejects the null hypothesis at the five percent level. However, it is interesting that "MORE" in Equation 4-24-7 does not respond to shocks with an equal and opposite magnitude of persistence: There is no evidence that either current or lagged DMRs cause an increase in the percentage of manufacturers reporting "more facilities needed." Thus an asymmetric persistence pattern is generated: While Barro-type shocks cause more manufacturers to be satisfied that they do not have too much capacity, such shocks do not cause manufacturers to believe that they need to add to their existing plant-and-equipment facilities. The failure of "MORE" to exhibit persistence is favorable to time-to-build, while the strong negative persistence displayed by "LESS" is, strictly speaking, inconsistent with the time-to-build mechanism.

Finally, Equations 4-24-9 and 4-24-10 investigate the hypothesis that Barro-type shocks cause manufacturers' planned expenditures for a given (future) quarter to deviate from actual expenditures, where such evidence would be a statistically significant and positive coefficient on DMR(t-k), where k is the number of quarters ahead for which the projection was made. There is no evidence of such an effect, for k equal to either one or two quarters. Further, there is no evidence that the ratio of planned to actual expenditures responds to Barro-type shocks with persistence, as neither individual coefficients nor F-statistics testing for joint significance are statistically significant at the five percent level. These findings represent some evidence that neither the cancellation/postponement option (in the case of a positive shock) nor the speeding up of capital-production (for a negative shock) is an empirically significant factor in accounting for the capital-accumulation behavior of firms.

Results Using Alternative Measures of Nominal-Demand Shocks

Results for the ten "starts" concepts derived using alternative measures of nominal-demand shocks are easily summarized. Findings are presented in Tables 4-25 and 4-26. In general, results using alternative money-shock concepts support the findings for Barro-type shocks. However, results using nominal-GNP shocks unambiguously fail to find a statistically significant role for lagged shocks in accounting for the behavior of "starts." Thus the time-to-build hypothesis, and Hypothesis 9, is not rejected if it is the case that, as researchers such as Gordon (1982) maintain, nominal-GNP shocks and not money shocks

TABLE 4-25: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS OLDER THAN T-K EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	L(ODUR)		L(ONDK)		L(CAM)	
	k=0	k=2	k=0	k=2	k=0	k=2
DMR	1.78	1.99*	2.18*	2.29**	1.17	1.05
DMRG	1.94*	2.16*	2.66**	2.86**	1.17	1.18
DMRM	3.39**	3.61**	4.01**	4.30**	1.88*	1.99*
DMRN	1.53	1.72	2.22*	2.24*	1.12	0.97
DBR	1.40	1.42	1.60	1.39	0.60	0.63
DYRG	1.09	1.18	1.17	1.14	(1.18)	(1.33)
DYRM	1.03	1.10	1.22	1.25	1.03	1.13

TABLE 4-25--continued

Shock Concept	L(OCON)		L(HS)		L(OH)	
	k=0	k=2	k=0	k=1	k=0	k=1
DMR	(3.50)**	(3.96)**	2.03*	1.92*	2.35**	2.16*
DMRG	(2.26)*	(2.57)*	1.99*	1.95*	1.92*	1.77*
DMRM	(3.20)**	(3.10)**	(3.76)**	(3.47)**	(4.76)**	(3.78)**
DMRN	(3.90)**	(4.24)**	1.85*	1.73	1.84*	1.69
DBR	2.76**	2.34**	1.29	1.30	1.42	1.37
DYRG	1.19	1.28	(1.49)	(1.59)	1.18	1.23
DYRM	(1.24)	(1.32)	(1.22)	(1.30)	0.96	1.01

TABLE 4-25--continued

Shock Concept	"MORE"	"LESS"
	k=0	k=0
DMR	1.34	(2.12)*
DMRG	1.44	(1.96)*
DMRM	1.30	(4.25)**
DMRN	1.63	(2.55)*
DBR	1.28	(1.00)
DYRG	1.39	0.74
DYRM	0.88	(1.72)

TABLE 4-25--continued

Shock Concept	PLANONE	PLANTWO
	k=0	k=0
DMR	0.54	(0.75)
DMRG	0.80	(1.34)
DMRM	(0.70)	(1.38)
DMRN	0.80	1.33
DBR	0.48	1.27
DYRG	(1.16)	(0.88)
DYRM	1.52	1.90*

Notes: See Table 4-1 for definitions of variables. For each dependent variable, where k=0 all "relevant" lagged shocks are restricted to zero, while for k=i, all "relevant" lagged shocks older than i periods are restricted to zero.

TABLE 4-25--continued

Where k=0, all equations except for those using the DMRN shock concept have the same sample periods as listed in the notes to Table 4-24. For DMRN, all samples are for 1958:I-1979:III, except for "MORE" and "LESS," which are identical to the periods listed in the notes to Table 4-24. Where k=i, the corresponding degrees of freedom equal F(n-i,d), where n is the degrees of freedom for the F-statistic numerator where k=0, and d is the degrees of freedom for the denominator.

For the definition of "relevant" in the present context, see the discussion on pp. 226-228, above. F-statistics in parentheses leave one or more lagged shocks unrestricted; others restrict all lagged shocks to zero. Following is a list of: (a) F-statistic degrees of freedom, and, (b) lagged shocks left unrestricted, for all Fs in parentheses (presented in the general form F(n,d) {·}, where within brackets are the numbers corresponding to the shock logs left unrestricted). That is, the first entry below, for LOCON, means: F degrees of freedom are 12 in numerator and 75 in denominator, and DMR13, DMR14, DMR15, and DMR16 were left unrestricted (since their coefficients were both "wrongly-signed" and significant at the five percent level).

First, in the case of Fs where k=0: for DMR: L(OCON) F(12,75),{13-16}; "LESS" F(12,36),{10-13}; PLANTWO F(13,71),{10,11,15}.

For DMRG: L(OCON) F(11,75),{11,13-16}; "LESS" F(12,36),{10-13}; PLANTWO F(15,71),{3}.

For DMRM: L(OCON) F(15,75),{16}; L(HS) F(11,75),{7-11}; L(OH), F(8,75),{5-12}; "LESS" F(15,36),{10}; PLANONE F(14,71),{7,11}; PLANTWO F(14,71),{3,4}.

For DMRN: L(OCON) F(15,75),{16}; "LESS" F(13,36),{10-12}.

For DBR: "LESS" F(15,36),{11}.

For DYRG: L(CAM) F(14,75),{7,8}; L(HS) F(12,75),{3-6}; "MORE" F(14,36),{7,9}; PLANONE F(13,71),{1-3}; PLANTWO F(13,71),{1-3}.

For DYRM: L(OCON) F(15,75),{11}; L(HS) F(13,75),{3}; "LESS" F(6,36),{2-11}.

Second, in the cases where k=1 or k=2: for DMR: L(OCON) F(10,75),{1,2,13-16}. For DMRG: L(OCON) F(9,75),{1,2,11,13-16}. For DMRM: L(OCON) F(13,75),{1,2,16}; L(HS) F(10,75),{1,7-11}; L(OH) F(7,75),{1,5-12}. For DMRN: L(OCON) F(13,75),{1,2,16}. For DYRG: L(CAM) F(12,75),{1,2,7,8}; L(HS) F(11,75),{1,3,4-6}. For DYRM: L(OCON) F(13,75),{1,2,11}; L(HS) F(12,75),{1,3-5}.

F-statistics corresponding to the above which restrict all 16 lagged shocks to zero are presented in Table 4-26.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-26: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS OLDER THAN T-K EQUAL ZERO, QUARTERLY DATA, 1954:I-1979:III SAMPLE, SEVEN SHOCK CONCEPTS

Shock Concept	L(ODUR)		L(ONDK)		L(CAM)	
	k=0	k=2	k=0	k=2	k=0	k=2
DMR						
DMRG						
DMRM						
DMRN						
DBR						
DYRG					1.24	1.41
DYRM						

TABLE 4-26--continued

Shock Concept	L(OCON)		L(HS)		L(OH)	
	k=0	k=2	k=0	k=1	k=0	k=1
DMR	2.96**	3.05**				
DMRG	2.02*	2.06*				
DMRM	3.21**	2.99**	3.42**	3.23**	3.76**	3.41**
DMRN	3.81**	4.05**				
DBR						
DYRG			1.47	1.57	1.18	1.23
DYRM	1.44	1.59	1.25	1.33		

TABLE 4-26--continued

Shock Concept	"MORE"	"LESS"
	k=0	k=0
DMR		2.51**
DMRG		2.06*
DMRM		4.02**
DMRN		2.99**
DBR		1.31
DYRG	1.49	
DYRM		1.58

TABLE 4-26--continued

Shock Concept	PLANONE	PLANTWO
	k=0	k=0
DMR		1.58
DMRG		1.44
DMRM	1.01	1.42
DMRN		
DBR		
DYRG	1.46	1.44
DYRM		

Notes: See Table 4-1 for definitions of variables. For each dependent variable, where k=0 all lagged shocks are restricted to zero, while for k=i, all lagged shocks older than i periods are restricted to zero (omitted Fs are reported in Table 4-25).

TABLE 4-26--continued

Where k=0, all equations except for those using the DMRN shock concept have the same sample periods as listed in the notes to Table 4-24. For DMRN, all samples are for 1958:I-1979:III, except for "MORE" and "LESS," which are identical to the periods listed in the notes to Table 4-24. Where k=i, the corresponding degrees of freedom equal F(n-i,d), where n is the degrees of freedom for the F-statistic numerator where k=0, and d is the degrees of freedom for the denominator.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

are the appropriate shock variables in a Barro-type reduced-form regression equation. However, the strong rejection of Hypothesis 9 for the large majority of those equations using money shocks as explanatory variables is important in light of the fact that most of the empirical evidence favoring the policy-neutrality hypothesis has utilized money shocks in this capacity. Evidently a simple Kydland-Prescott type of time-to-build mechanism cannot fully account for such a persistent response by GNP and its components.

Results Stemming from the Disaggregation of Real Inventory Stocks

In the empirical policy-neutrality literature, the analysis of the persistence of real inventory stocks has been carried out with the intent of determining the consistency of the Blinder-Fischer hypothesis with the data (as, for example, in Demery and Duck, 1984). Since the Blinder-Fischer mechanism does not imply that a disaggregation of total stocks would be meaningful, analysis has been confined to the behavior of total inventories. Previous studies, however, have not taken advantage of the restrictions imposed by time-to-build on the persistence of inventories--restrictions which allow the imposition of tests that directly compare the explanatory power of time-to-build with that of Blinder-Fischer. These restrictions are not to be imposed on total inventory stocks themselves but rather stem from the disaggregation of inventories. Results of this analysis are reported in the present section.

Analysis is carried out for quarterly U.S. data over the 1958:IV-1979:III sample, where the starting date is the earliest

date at which unpublished Commerce Department data are available which disaggregate manufacturing inventories by stage of production. Findings are presented in Tables 4-27 through 4-29. Results suggest that time-to-build plays an important role (via the mechanism described in Chapter 3, p. 134) in generating the considerable persistence exhibited by total real inventory stocks in response to nominal-demand shocks. The impact of time-to-build appears to be concentrated in manufacturers' work-in-progress inventories. However, time-to-build cannot account for all of the persistence of inventories, and the remainder, concentrated in retailers' stocks, appears to be the result of the Blinder-Fischer mechanism.[23]

Results Using Barro-Type Money Shocks

Results using Barro-type money shocks are reported in Table 4-27 and in the first row of Table 4-29. First, total inventory stocks are disaggregated into manufacturers' stocks and the stocks of middlemen. Manufacturers' stocks are further disaggregated by stage of production, into finished-goods

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[23]An analysis parallel to that presented in this section also was carried out for inventory changes, where the dependent variable is of the form (DINVYX/Y)·L(Y), where DINVYX is some component of total inventories. However, results are of little interest in light of the fact that (DINVY/Y)·L(Y) displays little persistence over the 1958:IV-1979:III sample. Only the two nominal-GNP-shock concepts, DYRG and DYRM, generate results where the null hypothesis of joint significance of all lagged shocks is rejected at the five percent level or better. Since the deletion of only five years of data distinguish the present sample from the one yielding the results reported previously for DINVY, this finding is further evidence of the relative weakness of the relationship between inventory changes and nominal-demand shocks. Further, little persistence was generated by any of the component equations. Accordingly, the results of this work are not presented.

inventories (FIG), work-in-progress inventories (WIP), and materials-and-supplies inventories (MAS) (Equations 4-27-2 through 4-27-4). Stocks held by middlemen are further disaggregated by type of holder, into wholesalers' stocks (WH) and retailers' stocks (RET) (Equations 4-27-5 and 4-27-6). Second, the above categories (except for MAS) are further disaggregated into their durable and nondurable components (Equations 4-27-7 through 4-27-14). Third, durable retail stocks are disaggregated into their auto and nonauto components as a means of investigating the source of the persistence displayed by retail durable stocks (Equations 4-27-15 and 4-27-16). Tables 3-11 through 3-13 in Chapter 3 give data on the relative importance of several of these categories in determining total inventory stocks.

Equation 4-27-1 is the total inventories equation. Previous studies (Haraf, 1983, Demery and Duck, 1984, reviewed in Chapter 2, pp. 57-62, above) have found total stocks responding to money shocks with short-term, negative persistence, but have not found evidence of positive persistence. By contrast, in the present study strong and long-lasting positive persistence is displayed in response to Barro-type shocks, while short-term negative persistence is not in evidence. DMR3 through DMR16 are (positive and) statistically significant at the five percent level or better, and the data suggest that persistence of INVYS actually extends beyond 16 quarters. The F-test for the joint significance of all 16 lagged DMRs rejects the null hypothesis of no joint significance at the one percent level.

TABLE 4-27: QUARTERLY EQUATIONS FOR REAL INVENTORY STOCKS AND THE COMPONENTS OF REAL INVENTORY STOCKS, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1958:IV-1979:III U.S. DATA, "L(X)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF INVENTORIES)

Explanatory Variable	(1) INVYS	(2) FIG	(3) WIP	(4) MAS	(5) WH	(6) RET
Constant	5.37	2.80	2.28	3.36	3.33	4.66
	(45.6)	(14.0)	(8.6)	(9.7)	(13.7)	(26.1)
t	0.01	0.01	0.01	0.01	0.01	0.01
	(89.4)	(40.8)	(42.4)	(25.9)	(47.2)	(74.6)
LFG	0.01	0.20	0.24	0.03	-0.05	-0.25
	(0.2)	(4.2)	(4.0)	(0.3)	(-0.8)	(-6.1)
DMR	0.07	0.08	0.13	-0.18	-0.19	0.38
	(0.4)	(0.3)	(0.4)	(-0.4)	(-0.6)	(1.1)
DMR1	0.31	0.03	0.47	-0.15	-0.01	0.81
	(1.4)	(0.1)	(1.0)	(-0.3)	(-0.02)	(1.9)
DMR2	0.46	0.06	0.90	0.13	0.45	0.85
	(1.7)	(0.1)	(1.7)	(0.2)	(0.9)	(1.7)
DMR3	0.70	-0.15	1.67	0.09	0.78	1.79
	(2.4)	(-0.3)	(2.9)	(0.1)	(1.3)	(3.4)
DMR4	1.16	0.19	2.65	0.47	1.17	2.43
	(3.9)	(0.4)	(4.4)	(0.5)	(1.8)	(4.6)
DMR5	1.50	0.75	3.32	0.76	1.22	3.02
	(4.9)	(1.5)	(5.4)	(0.8)	(1.9)	(5.9)
DMR6	1.68	1.22	4.39	0.66	1.37	2.91
	(5.8)	(2.5)	(7.4)	(0.7)	(2.2)	(6.2)
DMR7	1.79	1.50	4.38	0.47	1.57	2.81
	(6.3)	(3.2)	(7.3)	(0.5)	(2.5)	(6.3)
DMR8	1.78	1.85	4.19	0.16	1.54	2.73
	(6.4)	(3.9)	(6.9)	(0.2)	(2.4)	(6.5)
DMR9	1.75	1.87	4.21	0.10	1.74	2.34
	(6.5)	(4.0)	(7.2)	(0.1)	(2.8)	(5.8)
DMR10	1.54	1.63	3.78	0.11	1.25	2.13
	(5.9)	(3.7)	(6.8)	(0.1)	(2.1)	(5.5)
DMR11	1.28	1.36	3.71	-0.27	1.16	1.70
	(5.0)	(3.1)	(6.9)	(-0.3)	(2.0)	(4.3)
DMR12	1.12	1.23	3.28	-0.23	1.02	1.38
	(4.3)	(2.7)	(6.1)	(-0.3)	(1.7)	(3.3)
DMR13	0.95	0.96	2.47	-0.11	1.00	1.19
	(3.7)	(2.1)	(4.8)	(-0.1)	(1.8)	(2.8)
DMR14	0.89	1.11	1.92	-0.01	0.75	1.33
	(3.7)	(2.5)	(4.0)	(-0.02)	(1.5)	(3.1)
DMR15	0.78	0.96	1.09	0.45	0.52	0.82
	(3.6)	(2.4)	(2.5)	(0.7)	(1.2)	(2.1)
DMR16	0.50	0.67	0.64	0.09	0.22	0.75
	(3.2)	(2.3)	(1.9)	(0.2)	(0.7)	(2.3)

TABLE 4-27--continued

Explanatary Variable	(1) INVYS	(2) FIG	(3) WIP	(4) MAS	(5) WH	(6) RET
a1	0.93	0.94	0.86	1.00	0.99	0.65
	(7.0)	(7.0)	(6.4)	(7.5)	(7.6)	(5.0)
a2						
a3						
a4					-0.28	
					(-1.5)	
a5				0.30		
				(1.6)		
a6						
a7						
a8					-0.23	-0.24
					(-1.7)	(-1.9)
R^2	0.9996	0.998	0.999	0.997	0.999	0.999
s	0.006	0.01	0.01	0.02	0.01	0.01
Q (6 lags)	4.02	6.03	6.30	3.96	2.12	1.03
Q (24 lags)	29.03	16.19	41.82**	25.96	13.91	25.90
F(16,56)	4.47**	1.70	5.23**	0.35	1.04	6.90**

TABLE 4-27--continued

Explanatory Variable	(7) FIGDU	(8) FIGND	(9) WIPDU	(10) WIPND	(11) WHDU	(12) WHND
Constant	2.29	1.78	2.02	1.00	2.87	2.44
	(11.7)	(6.4)	(6.4)	(4.3)	(11.9)	(5.8)
t	0.01	0.01	0.01	0.01	0.01	0.01
	(46.3)	(28.5)	(36.1)	(51.9)	(58.1)	(23.5)
LFG	0.14	0.29	0.26	0.15	-0.09	0.003
	(3.1)	(4.4)	(3.5)	(2.7)	(-1.6)	(0.03)
DMR	0.23	-0.03	0.18	0.05	-0.41	0.51
	(0.7)	(-0.1)	(0.5)	(0.1)	(-1.4)	(0.9)
DMR1	-0.15	0.24	0.47	0.67	-0.34	1.08
	(-0.3)	(0.4)	(0.9)	(1.4)	(-0.8)	(1.3)
DMR2	-0.61	0.74	1.08	0.44	0.03	1.32
	(-1.1)	(1.2)	(1.7)	(0.8)	(0.1)	(1.32)
DMR3	-0.67	0.34	1.92	0.93	0.32	1.51
	(-1.2)	(0.5)	(2.8)	(1.7)	(0.6)	(1.4)
DMR4	-0.06	0.45	3.11	1.00	0.84	1.78
	(-0.1)	(0.7)	(4.3)	(1.8)	(1.6)	(1.7)
DMR5	0.67	0.74	3.92	0.92	1.02	1.69
	(1.3)	(1.1)	(5.3)	(1.7)	(1.8)	(1.6)
DMR6	1.41	0.88	5.21	0.93	1.27	1.85
	(2.8)	(1.3)	(7.3)	(1.8)	(2.4)	(1.8)
DMR7	1.99	0.94	5.24	0.79	1.56	2.13
	(4.0)	(1.4)	(7.3)	(1.5)	(2.8)	(2.1)
DMR8	2.42	1.21	5.00	0.73	1.33	2.39
	(4.9)	(1.8)	(6.9)	(1.4)	(2.3)	(2.3)
DMR9	2.17	1.51	5.01	0.70	1.20	2.86
	(4.5)	(2.4)	(7.2)	(1.4)	(2.2)	(2.9)
DMR10	2.11	1.15	4.59	0.08	0.87	1.83
	(4.5)	(1.9)	(6.9)	(0.2)	(1.7)	(1.9)
DMR11	1.75	0.89	4.94	0.04	0.64	1.92
	(3.7)	(1.5)	(7.0)	(0.1)	(1.3)	(2.0)
DMR12	1.40	0.85	3.99	-0.09	0.40	1.91
	(2.9)	(1.4)	(6.2)	(-0.2)	(0.8)	(2.0)
DMR13	1.31	0.46	3.02	-0.26	0.67	1.84
	(2.6)	(0.8)	(4.8)	(-0.6)	(1.4)	(1.9)
DMR14	1.58	0.63	2.29	0.19	0.51	1.88
	(3.2)	(1.1)	(4.0)	(0.4)	(1.1)	(2.1)
DMR15	1.44	0.51	1.30	-0.06	0.53	1.18
	(3.0)	(1.0)	(2.5)	(-0.1)	(1.3)	(1.4)
DMR16	0.84	0.60	0.70	0.15	0.28	0.55
	(2.4)	(1.5)	(1.8)	(0.4)	(1.0)	(1.0)

TABLE 4-27--continued

Explanatory Variable	(7) FIGDU	(8) FIGND	(9) WIPDU	(10) WIPND	(11) WHDU	(12) WHND
a1	0.92	0.92	0.85	0.74	0.89	1.02
	(7.1)	(6.8)	(6.4)	(5.8)	(6.9)	(7.7)
a2						
a3						
a4						
a5						
a6						
a7						
a8	-0.22			-0.30	-0.28	
	(-1.7)			(-2.4)	(-2.2)	
R^2	0.997	0.996	0.998	0.998	0.999	0.996
s	0.01	0.01	0.02	0.01	0.01	0.02
Q (6 lags)	1.80	8.07	7.41	1.35	0.55	1.25
Q (24 lags)	16.92	25.68	47.56**	16.05	11.78	23.25
F(16,56)	2.99**	0.80	5.19**	0.78	1.27	1.03

TABLE 4-27--continued

Explanatory Variable	(13) RETDU	(14) RETND	(15) RETDUAUT	(16) RETDU-AUT
Constant	4.59	3.35	2.94	3.77
	(11.4)	(16.4)	(4.9)	(9.8)
t	0.01	0.01	0.02	0.01
	(40.3)	(36.7)	(34.4)	(29.1)
LFG	-0.54	-0.003	-0.39	-0.44
	(-5.7)	(-0.1)	(-2.8)	(-4.9)
DMR	0.71	0.19	1.70	-0.32
	(1.0)	(0.7)	(1.1)	(-0.7)
DMR1	1.36	0.39	1.53	0.44
	(1.4)	(1.0)	(0.8)	(0.7)
DMR2	0.98	0.83	0.90	0.13
	(1.0)	(1.7)	(0.5)	(0.2)
DMR3	2.86	1.27	4.33	0.77
	(2.7)	(2.4)	(2.3)	(1.0)
DMR4	3.58	1.82	6.23	0.57
	(3.4)	(3.4)	(3.4)	(0.7)
DMR5	4.72	2.02	7.82	1.27
	(4.5)	(3.8)	(4.3)	(1.4)
DMR6	4.12	2.20	5.99	1.57
	(4.2)	(4.4)	(3.4)	(1.8)
DMR7	3.57	2.43	5.28	1.44
	(3.7)	(4.9)	(3.1)	(1.6)
DMR8	3.56	2.27	4.95	1.64
	(3.7)	(4.6)	(2.9)	(1.8)
DMR9	3.14	1.85	4.45	1.34
	(3.4)	(3.9)	(2.7)	(1.5)
DMR10	2.75	1.79	4.61	0.62
	(3.1)	(3.9)	(2.8)	(0.7)
DMR11	2.17	1.36	3.25	0.74
	(2.4)	(3.0)	(2.0)	(0.9)
DMR12	1.39	1.31	1.68	0.36
	(1.5)	(2.8)	(1.0)	(0.5)
DMR13	1.21	1.11	0.77	0.67
	(1.3)	(2.4)	(0.5)	(0.9)
DMR14	1.48	1.08	1.72	0.63
	(1.6)	(2.5)	(1.0)	(0.9)
DMR15	0.64	0.79	-0.96	1.27
	(0.7)	(2.0)	(-0.6)	(2.1)
DMR16	0.63	0.72	0.31	0.53
	(0.9)	(2.6)	(0.2)	(1.2)

TABLE 4-27--continued

Explanatory Variable	(13) RETDU	(14) RETND	(15) RETDUAUT	(16) RETDU-AUT
a1	0.66	0.95	0.57	0.85
	(5.0)	(7.1)	(4.3)	(6.4)
a2				
a3				
a4				0.28
				(1.6)
a5				
a6				
a7				
a8				
R^2	0.996	0.998	0.99	0.998
s	0.03	0.01	0.06	0.02
Q (6 lags)	0.53	7.46	0.22	4.11
Q (24 lags)	21.07	24.69	23.33	12.61
F(16,56)	2.30**	2.74**	2.52**	1.10

Notes: See Table 4-1 for definitions of variables. t-statistics for each coefficient are in parentheses immediately below the coefficient. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. Only values for ai with t-statistics of 1.5 or greater are reported. F-tests reported test for the joint statistical significance of all lagged DMRs.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

Analysis of persistence of the major components of inventories: Hypothesis 10

Equations 4-27-2 through 4-27-6 present the results stemming from the "first-order" disaggregation of inventories. From these equations the consistency of the data with Hypotheses 10 and 11 can be assessed. Beginning with Hypothesis 10, part [A] of that hypothesis asserts that, if time-to-build plays an important role in generating the persistence of inventories, then {the log of} WIP should display substantial persistence, while, if Blinder-Fischer plays an important role, then {the logs of} RET and/or WH ought to exhibit substantial persistence (as measured in both cases by the joint significonce of all lagged shocks). From the equations it is clear that, at least where Barro-type shocks are used, the data are consistent with either hypothesis.

Beginning with time-to-build, work-in-progress inventory stocks display a very pronounced persistence in response to DMRs. The pattern of significant DMRs begins with DMR3, rises to a peak at DMR6 and DMR7, and tapers off slowly through DMR15. It is interesting that a statistically significant effect does not appear until three quarters after the date of the shock, a result which is consistently observed across all of Table 4-27 and which therefore seems too general to be chiefly attributable to lengthy average production periods. In sum, the very substantial persistence displayed by WIP is in accordance with what would be predicted by time-to-build. There is, however, evidence of serial correlation in the residuals of Equation 4-27-3, as the Q-statistic for 24 lags rejects the null hypothesis of no serial correlation in the residuals at the one percent level. First-

differencing Equation 4-27-3 and reestimating assuming an eighth-order autoregressive process eliminated the evidence of serial correlation, and revealed mixed evidence that the persistent response of WIP to Barro-type shocks is robust to first-differencing: DMR4 through DMR14 remain significant at the five percent level or lower, and the same type of coefficient pattern is obtained. However, the F-test for joint significonce of all lagged DMRs now fails to reject the null hypothesis, although this failure to reject is marginal, the F-stotistic equalling 1.81 versus a critical F(16,56) of about 1.84.

Turning now to Blinder-Fischer and the behavior of the stocks of middlemen, the outstanding finding is the strong persistence displayed by retail inventories. The pattern of statistically significant DMRs begins at DMR3, rises to a peak at DMR5, and continues at least through DMR16, and the F-test for the joint significance of the 16 lagged DMRs rejects the null hypothesis at the one percent level. While results for RET are thus highly consistent with Blinder-Fischer, results for wholesalers' stocks are much less so. Here only DMR6 through DMR11 are statistically significant, and the F-test for joint significance fails to reject the null hypothesis. Thus wholesalers' inventories do not respond to Barro-type shocks with a statistically significant amount of persistence. This latter finding is a mild contradiction of Blinder-Fischer in the absence of an explanation as to why retailers' and wholesalers' stocks should respond differently to shocks. However, results overall for middleman inventories support the Blinder-Fischer hypothesis.

It is convenient to discuss here the results for the other two components of manufacturers' inventories, FIG and MAS (Equations 4-27-2 and 4-27-4, respectively), even though they do not figure in the evaluation of part [A] of Hypothesis 10. Neither of these categories exhibits a statistically significant response to shocks as measured by the F-test for the joint significance of all lagged DMRs. For the case of MAS, no individual coefficient is significant either, so that Barro-type shocks unambiguously fail to elicit a statistically significant response from materials-and-supplies inventories. For the case of FIG, results are more complex. Statistically significant DMRs begin with DMR6--a year and a half after the shock--and continue at least through DMR16, peaking in effect nine quarters after the shock. As previously discussed (note 21, above), individual coefficients here can be argued to be of interest in view of the nearness of the F-statistic to the critical F of 1.80.[24]

Time-to-build predicts that WIP should show the "most" persistence while Blinder-Fischer predicts that RET and/or WH should show the "most" persistence. In previous sections where it has been important to compare the magnitudes of persistence exhibited by various dependent variables, the F-test for joint

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[24]It is interesting to speculate on the extent to which the persistence of FIG also can be attributed to time-to-build in light of the results reported in Table 4-27. One can imagine an interaction where goods upon their completion are placed in FIG--one would then expect the relation between WIP and FIG that is actually observed, where FIG begins showing persistence somewhat later than WIP and then continues its response to shocks several quarters after WIP has ceased to show persistence. However, it is difficult to draw definite conclusion without (much) more specific information regarding the composition of WIP and FIG and the forces leading goods to be placed in these categories.

significance of lagged shocks, supplemented by knowledge of the statistical significance and pattern of individual coefficients on the DMR, has been sufficient to determine relative magnitudes. However, where dependent variables are (the logs of) inventories or the major components of inventories, patterns and magnitudes of persistence are similar enough to make useful a summary measure of the persistence displayed by each category. In particular, it is difficult without such a measure to conclude whether WIP or RET displays the "most" persistent response to Barro-type shocks. Further, given that the persistence of FIG occurs later than that of WIP (presumably "late" persistence should be counted more heavily than "early" persistence), it is not entirely clear that WIP exceeds FIG in magnitude of persistence revealed. A useful summary measure of persistence is the "Z-coefficient," defined in the present context as

$$Z_i = (f_i/16)\cdot[(b_{i1}/B_i)\cdot 1 + (b_{i2}/B_i)\cdot 2 + \ldots + (b_{ij}/B_i)\cdot j + \ldots + (b_{iM}/B_i)\cdot M].$$

Here Z_i is the Z-coefficient for inventory category i, f_i is the number of the 16 lagged DMR coefficients that is statistically significant for the category-i equation, b_{ij} is the statistically significant coefficient on DMRj for cotegory i, B_i is the sum of the stotistically significant coefficients on the lagged DMR for category i, and M is the last quarter following the shock for which a statistically significant response remains (out of 16) for category i--that is, DMR(M) is the oldest shock to be statistically significant in the category-i equation. (For

purposes of calculating the Z-coefficient, a "statistically significant" coefficient is defined to be one with a t-statistic of 1.5 or greater.) Thus the Z-coefficient weights persistence more highly the later after the shock the persistence occurs (but at a decreasing rate), and weights persistence more highly according to the frequency--the proportion of significant coefficients--with which it occurs. However, the coefficient does not reward a larger total effect as measured by the sum of the coefficients in a given equation. Nor does it place more weight on significant DMR coefficients with larger t-statistics.

The first row of Table 4-29 (pp. 379-380, below) presents Z-coefficients for INVYS and its major components (except for MAS, for which Z is undefined). Further, since Z does not take into account the comparative size of coefficients, Table 4-29 also presents the sums of statistically significant DMRs for each of the categories under analysis. Using these two measures of the magnitude of persistence, parts [B] and [C] of Hypothesis 10 can be evaluated.

Hypothesis 10[B] asserts that, if time-to-build is the dominant cause of the persistence of inventories, then: [B1] "more" persistence should be displayed by WIP than by FIG, WH, or RET; and, conversely, if Blinder-Fischer is the dominant cause, then: [B2] RET, WH, and FIG (all of which are composed of finished goods and presumably more susceptible to Blinder-Fischer) should show "more" persistence than WIP (composed of intermediate goods and therefore presumably less susceptible to the effect). For Barro-type shocks, results support [B1] and do

not support [B2], as Z_2 is slightly larger than Z_1 and Z_4 and substantially larger than Z_3 (however, since DMR16 is significant for both FIG and RET, there is the possibility of additional significant coefficients for these categories, which if counted might alter the results). Further, the sum of the coefficients in the WIP equation is substantially higher than that for any other equation. Thus, whether the measure of the magnitude of persistence is the Z-coefficient or the sum of all "significant" lagged DMR coefficients, results support the proposition that the magnitude of persistence is greatest for WIP.

Hypothesis 10[C] asserts that, if time-to-build is the dominant cause of inventory persistence, then: [C1] the "least" persistence should be displayed by RET (the least durable and least likely to be composed of goods with lengthy production periods); and, conversely, if Blinder-Fischer is the dominant cause of persistence, then: [C2] RET should exhibit the "most" persistence in response to nominal-demand shocks. Hypothesis 10[C] is thus one which draws the sharpest possible distinction between the two hypotheses. The evidence from row 1 of Table 4-29 supports neither [C1] nor [C2]: Z_4 is much larger than would be likely if time-to-build were the only mechanism at work in the data, but Z_4 is smaller than Z_2, suggesting that Blinder-Fischer is also not the only mechanism at work in the data. A similar verdict is reached if the measure of the magnitude of persistence is the sum of the significant lagged DMR coefficients.

Analysis of the short-term persistence pattern of finished-goods stocks: Hypothesis 11

Returning to Table 4-27, the consistency of the data with Hypothesis 11 can be assessed. Hypothesis 11 asserts that, if the Blinder-Fischer mechanism plays a major role in generating the persistence of inventories, then stocks of finished goods ought to show short-term negative persistence. This prediction stems from the basic premise of the Blinder-Fischer mechanism: that a positive (negative) shock causes a short-term pickup (decline) in demand which is initially met out of inventories, so that the short-term movement in such stocks should be inversely related to the shock. Such a response might be difficult to pick up in a total-inventories equation, but ought to show up in the various finished-goods equations which stem from the disaggregation of total inventories. However, an inspection of Table 4-27 reveals no such tendency for any category of finished-goods stocks. Not only do FIG, WH, and RET fail to exhibit any short-term negative persistence, but also the regressions stemming from the further disaggregations of these categories fail to exhibit such a response. These findings cast some doubt on the extent to which the persistent response of RET to Barro-type shocks can be attributed to a Blinder-Fischer type of effect.

Analysis of persistence in durable versus nondurable stocks: Hypothesis 12

A final source of discriminating tests capable of pitting the explanatory power of time-to-build against that of Blinder-Fischer is Hypothesis 12, which stems from the disaggregation of

each of the major components of total inventory stocks into their durable and nondurable portions. Time-to-build, which associates persistence in inventories with lengthy production periods, predicts that such persistence will be concentrated in the durable-goods portion of inventories. The Blinder-Fischer mechanism, which does not imply that a distinction between durable and nondurable stocks is meaningful, does not predict that durable stocks will respond to shocks differently than nondurable stocks. The relevant results for Barro-type shocks are presented in Equations 4-27-7 through 4-27-16.

Findings for manufacturers' inventories strongly support the previous conclusion that the persistence of this category chiefly is due to time-to-build. Equations 4-27-7 and 4-27-8 are the equations for, respectively, the durable (FIGDU) and nondurable (FIGND) portions of finished-goods manufacturers' inventories. Persistence clearly is confined to the durable portion of FIG. The F-test for joint significance rejects the null hypothesis at the one percent level, and the pattern of significant individual coefficients is very similar to that observed previously for FIG. By contrast, the nondurable portion of FIG shows no evidence of meaningful persistence: Only DMR9 is significant, and the F-test fails to reject the null hypothesis of no joint significance of the 16 lagged DMRs. In sum, the data strongly suggest that the durable-goods portion of FIG accounts for virtually all the persistence displayed by that category.

Results for work-in-progress manufacturers' inventories are of special interest due to the highly persistent response

previously observed for that category. Equations 4-27-9 and 4-27-10 are the equations for, respectively, the durable (WIPDU) and nondurable (WIPND) portions of WIP. The data indicate that all of the measurable persistence displayed by WIP is accounted for by the durable-goods portion of that category. The response of WIPDU to shocks is very similar to that of WIP, and the F-test rejects the null hypothesis of no joint significance of the 16 lagged DMRs at the one percent level. By contrast, no individual coefficient on a lagged shock is statistically significant in the WIPND equation (a remarkable result), and the F-test fails to reject the null hypothesis of no joint significance. As was true for WIP, however, there are problems with interpreting the WIPDU equation, due to the presence of serial correlation in the residuals (the Q-statistic for 24 lags rejects the null hypothesis of no serial correlation at the one percent level). First-differencing all variables in the WIPDU equation and reestimating (again assuming an eighth-order autoregressive process) eliminated the evidence of serial correlation and led to results similar to those when the WIP equation was reestimated: a long series of statistically significant lagged DMRs (DMR4 through DMR15), but an F-statistic which failed to reject the null hypothesis of no joint significance of all lagged shocks (F-statistic of 1.74 versus a critical F(16,56) of approximately 1.84).

Despite questions raised by serial correlation in the WIPDU equation, the data still indicate that, to the extent that work-in-progress manufacturing inventories exhibit a persistent

response to Barro-type shocks, the response is confined to the durable-goods portion of the category. Further, the data strongly indicate a similar verdict for the case of finished-goods manufacturing inventories. These results are favorable to the time-to-build-based explanation of inventory persistence.

Turning now to the results for middleman inventories, Equations 4-27-11 and 4-27-12 present the equations for, respectively, wholesalers' durable-goods inventories (WHDU) and wholesalers' nondurable-goods inventories (WHND). Results continue to indicate that wholesalers' stocks do not respond to Barro-type shocks with a statistically significant degree of persistence: While several individual coefficients are significant in both equations, neither F-test leads to a rejection of the null hypothesis of no joint significance of all lagged shocks. The pattern and frequency of statistical significance among individual lagged shock coefficients reveals, if anything, a more persistent response by nondurable than durable wholesalers' stocks. Results therefore do not suggest that a time-to-build type of mechanism is at work in the case of wholesalers' stocks, and, while results are somewhat more consistent with Blinder-Fischer for this category, the failure of either component of WH to exhibit substantial persistence does not support this latter hypothesis either.

Finally, results stemming from the disaggregation of retailers' stocks are presented as Equations 4-27-13 through 4-27-16. Equations 4-27-13 and 4-27-14 are, respectively, the equations for retailers' durable stocks (RETDU) and retailers'

nondurable stocks (RETND). Here results support the Blinder-Fischer mechanism and do not support time-to-build. Both durable and nondurable retailers' inventories respond to Barro-type shocks with a statistically significant amount of persistence: For both equations, the F-test for the joint significance of all lagged shocks rejects the null hypothesis at the one percent level. Moreover, as measured by individual coefficients, substantially more persistence is exhibited by the nondurable portion of retailers' stocks than is displayed by the durable portion, a result consistent with Blinder-Fischer but not with time-to-build.

Further evidence favoring the Blinder-Fischer explanation of RET persistence is presented in Equations 4-27-15 and 4-27-16, which explore the persistence of two components of RETDU; specifically, retailers' automobile stocks (RETDUAUT) and retailers' durable nonautomobile stocks (RETDU-AUT), respectively. Automobiles, a portion of retailers' inventories which does not have lengthy production periods, would not be expected to exhibit substantial persistence on time-to-build premises, while, by contrast, automobile stocks would be a component of inventories particularly susceptible to a Blinder-Fischer type of effect. The two regressions reveal that the data cannot reject the hypothesis that the persistence of durable retailers' inventories is confined to the automobiles portion of that category. Automobile stocks show substantial persistence, with an F-statistic that leads to a rejection of the null hypothesis of no joint significance at the one percent level, and

with a large number of significant individual shock coefficients. By contrast, only DMR15 is statistically significant in the RETDU-AUT equation, and the F-test fails to reject the null hypothesis of no joint significance of all lagged shocks. The results stemming from the disaggregation of retailers' stocks thus provide particularly strong evidence that the persistence of retailers' stocks is due to the Blinder-Fischer mechanism.

In sum, results stemming from the disaggregation of total inventory stocks support the hypothesis that both time-to-build and Blinder-Fischer are responsible for the considerable persistence exhibited by (the log of) INVYS to Barro-type money shocks. The persistence of manufacturers' inventories appears chiefly due to the time-to-build mechanism, whereas the persistence of retailers' stocks seems chiefly due to the Blinder-Fischer mechanism.

Results Using Alternative Measures of Nominal-Demand Shocks

Results derived using alternative nominal-demand-shock measures in general support the conclusions stemming from the analysis where Barro-type money shocks are employed as explanatory variables. The relevant results are presented in Tables 4-28 and 4-29. Table 4-28 gives F-statistics corresponding to the null hypothesis that all lagged shocks are statistically insignificant, for the seven shock concepts utilized in this study's previous quarterly work. Table 4-29 gives Z-coefficients and sums of "statistically significant" lagged shock coefficients for (the logs of) INVYS and its major components, for the seven quarterly shock concepts (again, for

TABLE 4-28: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND-SHOCK COEFFICIENTS EQUAL ZERO, 1958:IV-1979:III QUARTERLY U.S. DATA, SEVEN SHOCK CONCEPTS, "L(X)" FORM OF DEPENDENT VARIABLE (X IS SOME COMPONENT OF INVENTORIES)

Shock Concept	INVYS	FIG	WIP	MAS	WH	RET
DMR	4.47**	1.70	5.23**	0.35	1.04	6.90**
DMRG	4.75**	1.36	4.19**	0.54	1.11	3.88**
DMRM	1.12	0.80	1.27	0.57	0.87	1.52
DMRN	4.57**	1.13	4.78**	0.32	1.03	8.79**
DBR	1.66	0.91	2.67**	0.61	1.41	2.25*
DYRG	0.81	0.68	1.38	1.59	1.01	0.89
DYRM	1.94*	0.47	2.82**	1.12	2.24*	0.91

TABLE 4-28--continued

Shock Concept	FIGDU	FIGND	WIPDU	WIPND	WHDU	WHND
DMR	2.99**	0.80	5.19**	0.78	1.27	1.03
DMRG	2.52**	0.72	4.13**	0.70	1.15	1.05
DMRM	1.19	0.74	1.34	0.79	1.23	0.73
DMRN	2.04*	0.58	4.99**	0.73	1.30	0.89
DBR	0.90	0.97	2.79**	0.96	1.34	1.61
DYRG	0.54	1.04	1.34	0.50	0.63	2.18*[a]
DYRM	0.54	0.65	2.47**	3.13**	1.30	1.91*

TABLE 4-28--continued

Shock Concept	RETDU	RETND	RETDUAUT	RETDU-AUT
DMR	2.30*	2.74**	2.52**	1.10
DMRG	2.10*	1.59	1.88*	1.01
DMRM	1.11	1.19	1.32	0.90
DMRN	2.57**	2.53**	4.17**	1.25
DBR	1.49	2.54**	2.41**	0.47
DYRG	0.51	0.84	0.45	1.75
DYRM	1.04	1.06	0.70	1.56

Notes: See Table 4-1 for definitions of variables. F-tests reported test for the joint statistical significance of all lagged shocks.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]All statistically significant coefficients are negative.

TABLE 4-29: "Z-COEFFICIENTS" AND SUMS OF LAGGED SHOCK COEFFICIENTS, MAJOR COMPONENTS OF REAL INVENTORY STOCKS, 1958:IV-1979:III QUARTERLY U.S. DATA, SEVEN SHOCK CONCEPTS, "L(X)" FORM OF DEPENDENT VARIABLE (X EQUALS INVENTORIES OR SOME COMPONENT OF INVENTORIES)

Shock Concept	INVYS		FIG		WIP	
	Z	sum	Z_1	sum	Z_2	sum
DMR	8.14	17.88	7.11	14.36	8.05	42.60
DMRG	7.80	23.55	6.69	15.80	7.34	50.31
DMRM	3.23	8.16	und.	0.00	6.93	32.58
DMRN	8.44	10.74	7.47	7.79	8.11	25.30
DBR	7.94	15.01	2.78	7.59	8.12	40.09
DYRG	und.	0.00	5.39	8.26	4.99	7.43
DYRM	7.61	8.54	1.87	2.70	7.63	23.78
AVERAGE[a]	6.17	11.98	4.47	8.07	7.31	31.73

TABLE 4-29--continued

Shock Concept	WH		RET	
	Z_3	sum	Z_4	sum
DMR	5.93	13.79	7.36	28.99
DMRG	4.10	17.58	7.61	34.30
DMRM	0.25	1.09	1.55	8.50
DMRN	7.05	8.74	7.87	18.37
DBR	1.92	1.47	6.83	26.92
DYRG	und.	0.00	1.93	1.19
DYRM	6.41	10.07	3.60	2.73
AVERAGE[a]	3.67	7.53	5.25	17.29

Notes: See Table 4-1 for definitions of variables. "Z" is the Z-coefficient, defined on p. 368, above, while "sum" is the sum of selected lagged shock coefficients. In calculating "Z" and "sum," only positively-signed lagged shock coefficients with t-statistics of 1.5 or greater are utilized. "und." indicates where the coefficient is undefined.

[a]For purposes of calculating the average value of the Z-coefficient, where Z is undefined its value is set at zero.

purposes of calculating both "Z" and "sum", any lagged shock coefficient with a t-statistic of 1.5 or greater is taken to be "significant"). There is, first of all, substantial evidence that the persistence displayed by (the log of) total inventory stocks to Barro-type shocks is fairly robust across a number of shock concepts: Three out of five money-shock concepts generate an F-statistic that is significant at the one percent level, while one of the two nominal-GNP-shock concepts generates an F which implies a rejection of the null hypothesis (although at only the five percent significance level).

Turning now to Hypothesis 10[A], results are consistent with the hypothesis that persistence in manufacturers' inventories is chiefly due to time-to-build, while persistence in middleman inventories is chiefly due to Blinder-Fischer. When the criterion of measurement is the F-statistic, all of the statistically significant persistence in manufacturers' inventories continues to be confined to work-in-progress stocks: No statistically significant F-statistic is observed either for finished-goods stocks or materiols-ond-supplies stocks. The rejection of the null hypothesis is very robust for WIP, as four of five money-shock concepts, and five of seven nominal-demand-shock concepts, reject the null hypothesis for this category. However, the difficulties with serial correlation in the residuals of the WIP equation continue: All but one shock concept (DMRG) generates an equation for which the Q-statistic rejects the null hypothesis of no serial correlation at 24 lags, and four shock concepts reject the null hypothesis for six lags

as well. Retailers' stocks exhibit the most robust persistence after work-in-progress stocks, as four out of five money shock concepts elicit some amount of statistically significant persistence from RET as measured by the F-statistic (however, there is no indication that nominal-GNP shocks elicit such a response). Thus the previous finding for Barro-type shocks that the data cannot reject either Hypothesis 10[A1] or 10[A2] is robust to variation is nominal-demand-shock concept. Finally, wholesalers' stocks continue to fail to respond generally to shocks with persistence, as only one shock concept (DYRM) elicits such a response from (the log of) WH (and that only at the five percent level).

For Hypotheses 10[B] and 10[C], findings also confirm the previous results derived using Barro-type shocks. An investigation of Hypothesis 10[B] leads to the conclusion that WIP responds with stronger persistence than any other category when measured by the Z-coefficient and the sum of all significant coefficients; however, the problems with serial correlation in the WIP equation requires that this finding be discounted somewhat. For WIP, the average Z-value over the seven shock concepts is 7.31 and the average "significant" coefficient sum equals 31.73, numbers substantially higher than for the category with the second-highest total--RET, with Z-values averaging 5.25 and "significant" coefficient sums averaging 17.29. For both persistence measures, FIG and WH come in substantially below WIP

and RET.[25] Finding that Z_2 is greater than either Z_1, Z_3, or Z_4 supports time-to-build, but finding Z_4 to be as large as it is supports Blinder-Fischer. Thus, the previous finding for Hypothesis 10 where Barro-type shocks are used--that it is impossible to rule out the workings of either propogation mechanism in the data--is robust to a variation in shock concept.

Results allowing an assessment of Hypothesis 11 for the six additional shock concepts are not presented. However, inspection of the estimated equations for the various finished-goods inventory stocks reveals a general absence of negative short-term persistence in these equations, contradicting the prediction of the Blinder-Fischer mechanism. Thus this finding, previously reported where Barro-type shocks are used as explanatory variables, is robust to variation in shock concept. The robust rejection of Hypothesis 11 by the data, as before, raises some questions about whether the persistence of retail stocks can be attributed to the workings of the Blinder-Fischer mechanism.

Finally, Table 4-28 presents information relevant to a determination of how robust the previous findings are concerning Hypothesis 12 when various nominal-demand-shock concepts are utilized. Results turn out to be highly robust to this alteration. Both for finished-goods and work-in-progress

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[25]These findings on the whole are not sensitive to the possibility that some shocks older than 16 quarters are statistically significant. One of the FIG equations (DMRN) and two of the RET equations (DMRG and DMRN) might generate larger Z-coefficients if these equations were estimated with longer shock lags. However, four of the WIP equations (DMRN, DBR, DYRG, DYRM) would have to be reestimated as well, so that allowing for longer lags likely would tend to raise the average Z-value for WIP more than for FIG or RET.

manufacturers' inventories, no rejection of the null hypothesis of no joint shock significance occurs for nondurable stocks, whereas a number of strong rejections of this hypothesis occur for each of the durable components. Evidence of a statistically significant magnitude of persistence continues to be absent from wholesalers' inventories, with the single relevant exception being for WHND using Mishkin-type nominal-GNP shocks. Retailers' stocks exhibit fairly robust persistence with slightly more robustness being shown by RETND than by RETDU. Finally, the hypothesis that the persistence of RETDU is entirely due to the persistence of automobile inventories is strongly supported by the data. In sum, results stemming from use of alternative nominal-demand-shock concepts completely support the findings derived using Barro-type shocks.

Analysis of the Explanatory Power of the Wage-Stickiness Propagation Mechanism: Hypothesis 13

Previous sections have explored the extent to which the persistence of real GNP, its components, and related series can be accounted for by either the time-to-build propagation mechanism or the Blinder-Fischer inventories-based mechanism. A third propagation mechanism capable in theory of accounting for this persistence is the wage-stickiness mechanism advanced by Fischer (1977a). The wage-stickiness mechanism asserts that, for some economically-relevant period of time following a nominal-demand shock, product prices are more responsive to the demand shift than are wages paid to workers, so that over this interval a positive shock causes prices to rise relative to wages, and a

negative shock causes prices to fall relative to wages. The resulting rise in profits for a positive shock, and fall for a negative shock, tends to generate a persistent response by real output to nominal-demand shocks, a response which continues for as long as wage adjustment lags behind product-price adjustment.

The particular approach adopted in this study for purposes of testing the wage-stickiness mechanism already has been elaborated in the discussion accompanying Hypothesis 13. The main idea is a two-part procedure: first, to measure the magnitudes of wage-stickiness that exist for those major components of real GNP for which there is adequate data; and, second, to statistically compare the pattern of wage-stickiness for each GNP-component with the pattern of persistence exhibited by that component, with the object of determining the extent to which the wage-stickiness pattern accounts for the pattern of persistence exhibited by that GNP-component. Results of this procedure are presented for both annual and quarterly postwar U.S. data. Where annual data are utilized, analysis is carried out for nonresidential and residential construction, producers' durable equipment, and consumers' perishable goods. Where quarterly data are utilized, all of the above plus consumers' expenditures on services are investigated. Overall, results do not support the hypothesis that wage-stickiness is a major factor accounting for the persistence of real GNP.

As indicated in the discussion surrounding Hypothesis 13 (and in Chapter 2, pp. 66-67), the measure of wage-stickiness utilized in the present study is actually a measure of wage-

stickiness relative to product-price-stickiness, and is of the general form

$$d[L(P_i)] - d[L(W_i)],$$

where $d[\cdot]$ is the first-difference operator, P_i is a price measure for the ith component of GNP, and W_i is a wage measure relevant to the production of the ith component of GNP. For each GNP-component, the implicit price deflator corresponding to that component is taken as the measure of that component's price. The wage measure for each GNP-component is average hourly earnings for selected employees of that industry group which can most closely be associated with the particular GNP-component under analysis.

For both nonresidential and residential construction, W is average hourly earnings by "construction workers" in the construction industry (data giving separate figures for nonresidential- and residential-construction employees do not become available until 1972, and consequently are not utilized in this study). The U.S. Department of Labor (1982, p. 270) defines construction workers as, roughly, those working in construction at jobs usually performed by the construction trades, so that the measure excludes salaries of middle- and higher-level management. Use of the same earnings measure for both nonresidential and residential construction presumes that labor is easily substituted between the two branches of the construction industry, so that renumerations would tend to be equal across the two branches. For producers' duroble equipment, W is average

hourly earnings by "production and related workers" in the durable-goods manufacturing industries. Where the GNP-component is consumers' perishable goods, W is measured as average hourly earnings by "production and related workers" in the nondurable-goods manufacturing industry. Measures of average hourly earnings in both durable and nondurable manufacturing are heavily weighted towards "blue-collar" and lower-salary "white-collar" employees. Finally, where the GNP-component is consumers' expenditures on services, W is measured as average hourly earnings by "nonsupervisory employees" in the services industries, where such employees ore, again, primarily "blue-collar" and lower-salary "white-collar" workers.[26]

The use of hourly-earnings series as proxies for wage series requires additional discussion. One motive for using earnings as proxies for wages is that data on earnings are readily available in a form appropriate for combining with Commerce department sectoral price-index data. However, for present purposes an hourly-earnings measure is superior to a measure of "straight-time" wage rates. According to the Labor department: "Averages of hourly earnings differ from wage rates. Earnings are the actual return to the worker for a stated period of time; rates are the amounts stipulated for a given unit of work or time" (U.S. Department of Labor, 1982, p. 270). The distinction made in the above quote in principle includes two parts: first, a

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[26]Descriptions in the text of the types of jobs entering into the measures of durable goods, nondurable goods, and services are not close paraphrases but are rather summaries of detailed Labor Department descriptions. The source is U.S. Department of Labor (1982, p. 270).

distinction between a promised hourly-earnings figure and the actual figure; and, second, the distinction between a worker's hourly wage and various nonwage monetary rewards which accompany employment (and which, when combined with wages, add up to hourly earnings). Turning initially to the first of these, suppose thot there is a positive nominal-demand shock which generates a rise in product price in sector i. Suppose further that "straight-time" wage increases lag behind the rise in product price: This does not necessarily imply that, other things equal, the profit potential of the typical firm has risen as a result (which is the relevant point). For example, in order to meet rising demand for its product, the typical firm might be forced to increase overtime work and late-shift work and in other ways might be required to raise the ratio of "high-paid" work to "low-paid" work (while overtime and late-shift rates might be captured in an appropriately adjusted wage measure, other changes in the ratio alluded to above might not be so captured). What is really needed, that is, is not a measure of wage-stickiness so much as a measure of "cyclical labor-cost stickiness." The average hourly-earnings measure utilized in this study, unlike a measure of "straight-time" wage rates, captures these supplemental effects.

Regarding the nonwage monetary awards which accompony a worker's employment, the Labor deportment indicotes that

> the eornings series does not measure the level of total labor costs on the part of the employer since the following are excluded: Irregular bonuses, retrooctive items, payments of vorious welfare benefits, payroll taxes paid by employers, and earnings for those employees not covered under the production worker, construction worker, or nonsupervisory employee definitions. (U.S. Department of Labor, 1982, p. 270)

The exclusion of these items, however, should not generate meaningful distortions when the hourly-earnings measure is used for present purposes. Irregular bonuses and retroactive items should not be quantitatively important enough to cause problems; furthermore, as payments which are legally fully voluntary and economically at least partly so, bonuses should not be taken as part of required payments to employees. Payments of welfare benefits probably are not quantitatively important, and would not tend to raise payments to labor following a positive shock (or lower them following a negative shock); rather, they would have an effect opposite to the one causing concern in the present context. Payroll taxes paid per worker would not be expected to have a substantial cyclical component and can consequently be excluded without introducing distortions. Earnings of middle- and top-level management probably are, in general, not quantitatively important in the present context. Finally, a factor of substantial quantitative significance not mentioned by the Labor department--pension-fund obligations by firms--would, like payroll taxes, not be expected to have a substantial cyclical component (per worker employed). On these grounds, then, the average hourly-earnings measure utilized in this study can be taken as an appropriate proxy for wage-stickiness as that term is used in the context of the wage-stickiness propagation mechanism.

Annual Results

Results of carrying out the two-stage wage-stickiness test for annual data are presented in Tables 4-30 through 4-32. Table

4-30 presents annual wage-stickiness equations (actually the equations indicate the magnitude of wage-stickiness relative to price-stickiness) for nonresidential and residential construction, producers' durable equipment, and consumers' perishable goods, where Barro-type money-shocks are utilized as explanatory variables. All samples are for the 1948-85 period except for the second of the two nonresidential-structures equations, which is estimated over the 1954-85 period due to the problems previously reported with nonresidential structures over the 1946-85 period (pp. 229-232, above). Results allow an assessment of the extent to which Barro-type money shocks generate a rise in product price relative to wage rates,[27] for the four types of output under analysis. Further, results allow an assessment of the extent to which such shocks generate such a response for one or more years following a shock; that is, of the extent to which d[L(P)]-d[L(W)] responds with persistence in the annual data.

Equations 4-30-1 and 4-30-2 are the two nonresidential-structures equations, estimated over the 1948-85 and 1954-85 periods, respectively. Some evidence of wage-stickiness can be found, as the coefficient on DMR1 is both positive and statistically significant in both equations. However, no other shock coefficient elicits a statistically significant amount of wage-stickiness, and the F-statistic testing for the joint

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[27]Despite the fact that average hourly-earnings figures are being used as proxies for wage rates, in the discussion such measures will continue to be referred to as "wage rates," in keeping with the spirit of the Fischer (1977a) mechanism.

TABLE 4-30: ANNUAL "WAGE-STICKINESS" EQUATIONS FOR FIVE TYPES OF OUTPUT, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1948-85 U.S. DATA

	(1)	(2)	(3)	(4)	(5)
Explanatory Variable	P-WCONN	(1954-85) P-WCONN	P-WCONR	P-WDUR	P-WNDUR
Constant	0.05	0.67	0.10	0.12	-0.02
	(0.4)	(2.7)	(0.8)	(1.5)	(-0.3)
t	0.002	0.002	0.002	0.0002	0.001
	(3.5)	(4.0)	(4.0)	(0.6)	(2.5)
LFG	-0.03	-0.15	-0.05	-0.03	-0.008
	(-1.3)	(-3.2)	(-2.0)	(-1.7)	(-0.5)
DMR	0.09	0.20	0.47	-0.69	0.13
	(0.3)	(0.6)	(2.2)	(-3.3)	(0.6)
DMR1	0.72	0.80	0.49	-0.29	0.18
	(2.3)	(2.4)	(1.9)	(-1.4)	(0.8)
DMR2	-0.25	0.24	-0.19	0.23	-0.11
	(-0.8)	(0.7)	(-0.7)	(1.0)	(-0.5)
DMR3	-0.15	0.001	-0.06	0.14	-0.07
	(-0.5)	(0.003)	(-0.3)	(0.6)	(-0.3)
a1	0.43	0.28	0.69	0.30	0.38
	(2.4)	(1.4)	(3.8)	(1.7)	(2.4)
a2	-0.34	-0.38	-0.24	-0.25	-0.50
	(-1.9)	(-2.0)	(-1.3)	(-1.4)	(-3.1)
R^2	0.53	0.60	0.72	0.48	0.48
s	0.03	0.02	0.02	0.02	0.02
Q (6 lags)	1.99	6.15	1.04	5.33	6.31
F(3,29)	2.77	2.05	3.31*	1.37	0.39

Notes: See Table 4-1 for definitions of variables. t-statistics are in parentheses. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic

TABLE 4-30--continued

used to test for residual randomness. F-statistic tests for the statistical significance of all lagged DMRs.

*Estimate is statistically significant at the five percent level.

TABLE 4-31: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND SHOCKS EQUAL ZERO, ANNUAL "WAGE-STICKINESS" EQUATIONS FOR FIVE TYPES OF OUTPUT, EIGHT SHOCK CONCEPTS, VARIOUS PERIODS, 1948-85 U.S. DATA

	(1)	(2)	(3)	(4)	(5)
Shock Concept	P-WCONN	(1954-85) P-WCONN	P-WCONR	P-WDUR	P-WNDUR
DMR	2.77	2.05	3.31*	1.37	0.39
DMRGA	2.65	0.54	2.89	0.78	0.17
DMRGB	3.30*	3.60*	5.96**	2.09	0.86
DMRM	1.88	1.88[a]	1.04	0.90	2.69
DMRN	3.59*	2.20	3.41*	0.70	0.98
DBR	0.32	2.91	0.93	0.38	0.59
DYRG	3.66*	6.24**	3.45*	0.80	0.46[b]
DYRM	4.49*	7.55**	4.63**	0.76	1.69

Notes: See Table 4-1 for definitions of variables. F-statistics have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data are available for all lags, and all samples end in 1985): [1] In Columns 1,3,4, and 5: DMR, DMRGA, DMRGB, and DBR, F(3,29) {1948}; DMRM, F(3,17) {1960}; DMRN, DYRG, DYRM, F(3,26) {1951}. [2] In Column 2: DMRM, F(3,17) {1960}; all other shocks, F(3,23) {1954}.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]1960-85 sample.

[b]Regression from which F is derived leaves DYRG2 unrestricted, since its coefficient is both negative and significant--thus F(2,29) holds for this case. The corresponding F-statistic with all three lagged shocks restricted to zero is F(3,29)=2.33.

TABLE 4-32: F-STATISTICS TESTING NULL HYPOTHESIS THAT THE PATTERN OF RESPONSE OF X TO ALL CURRENT AND LAGGED NOMINAL-DEMAND SHOCKS IS IDENTICAL TO THE PATTERN OF "WAGE STICKINESS," EIGHT SHOCK CONCEPTS, VARIOUS PERIODS, 1946-85 ANNUAL U.S. DATA (WHERE X IS SOME COMPONENT OF GNP)

Shock Concept	(1954-85) STR (1)	STR (2)	STR (3)	STR (4)	H (5)	H (6)
DMR	0.06	0.26	1.51	1.81	2.13	1.78
DMRGA	1.27	1.33	0.62	0.70	2.95*	2.19
DMRGB	0.53	0.88	0.62	1.07	3.14*	3.45*
DMRM	3.94*	4.63*	3.94*[a]	4.63*[a]	0.50	0.17
DMRN	0.24	0.19	1.86	1.95	3.59*	2.56
DBR	0.41	0.41	0.77	1.24	0.26	0.05
DYRG	2.14	2.00	0.68	0.72	2.47	1.43
DYRM	1.68	1.55	0.34	0.36	5.08**	2.92

TABLE 4-32--continued

Shock Concept	PDUR (7)	PDUR (8)	CPER (9)	CPER (10)
DMR	5.88**	4.71**	2.27	0.99
DMRGA	2.31	1.87	3.36*	1.08
DMRGB	6.16**	5.14**	4.39*	3.30*
DMRM	1.81	1.87	0.63	1.34
DMRN	3.33*	3.15*	1.73	1.63
DBR	9.75**	10.85**	0.04	0.73
DYRG	3.32*	2.44	4.86**	0.58
DYRM	5.44**	3.89*	6.92**	0.10

TABLE 4-32--continued

Notes: For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated.

Where a particular pattern of predicted shock effect can be deduced from the "wage-stickiness" equations, F-statistics (except for in the "STR (1954-85)" equations) have the following degrees of freedom and stem from a regression estimated over the sample with the starting date indicated (starting date is the earliest year for which data is available for all lags, and all samples end in 1985): DMR, F(3,31) (1946); DMRGA and DMRGB, F(3,30) (1947); DMRM, F(3,17) (1960); DMRN, F(3,26) (1951); DBR, F(3,31) (1946); DYRG and DYRM, F(3,26) (1951). In the "STR (1954-85)" equations, F(3,23) (1954) holds for all equations except those utilizing DMRM, for which F(3,17) (1960) holds.

Where the "wage-stickiness" equations fail to indicate a statistically significant impact of shocks, F-statistic degrees of freedom are as described in the preceding paragraph except that numerator degrees of freedom equal four rather than three (since all lagged shocks and the contemporaneous shock are restricted to zero in such a case).

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]1960-85 sample.

significance of all three lagged shock terms fails to reject the null hypothesis of no joint significance in both equations. Thus, the evidence suggests that, at least where Barro-type money shocks are used as the shock concept, the amount of wage-stickiness exhibited by nonresidential construction is small in the annual data.

Equation 4-30-3 is the residential structures equation, where again results are mixed. Here the F-test suggests that the three lagged shocks do elicit a statistically significant (five percent level) amount of wage-stickiness from this category. However, no individual lagged shock is statistically significant in the equation, although DMR is significant and DMR1 just misses statistical significance. On balance, relatively more weight can be assigned to the F-test and it can be concluded that somewhat more wage-stickiness appears to exist in residential construction than in nonresidential construction.

Equations 4-30-4 and 4-30-5 are the two equations for producers' durable equipment and consumers' perishable goods, respectively. In an important result, there is no evidence of wage-stickiness in the producers'-durable-equipment equation, a result which suggests that it is unlikely that the considerable persistence exhibited by producer durables in the annual data can be attributed to the wage-stickiness mechanism. If anything, the results suggest that wage changes precede price changes for those goods making up producers' durable expenditures. The results for consumer perishables also indicate a general absence of wage-stickiness.

One additional result of interest in Table 3-30 is the general tendency for the time trend (t) to be statistically significant in the various equations. The trend is significant for three of the four GNP-components analyzed, the lone exception being producers' duroble expenditures. This result suggests that the degree of wage-stickiness in the U.S. economy has tended to increase over the 1948-85 sample (the significance of the trend also shows up in the quarterly equations, presented below).

Table 4-31 explores the robustness of the results reported in Table 4-30 to a variation in shock concept. Results are similar for the eight shock concepts utilized in the annual data: Some evidence exists of a statistically significant amount of wage-stickiness in construction, while there is no such evidence for manufacturing. Over the 1948-85 sample, two of six money-shock concepts, and four of eight nominal-demand-shock concepts, elicit a statistically significant response from nonresidential construction. Approximately the same magnitude of response is exhibited by nonresidential construction over the 1954-85 sample. For residential construction, a slightly stronger response is revealed, as three of six money-shock concepts, and five of eight nominal-demand-shock concepts, elicit statistically significant amounts of wage-stickiness. For all these equations, the response to nominal-GNP shocks is noticeably stronger than the response to money shocks. A similar robustness to variation in shock concept is revealed for producer durables and consumer perishables, as no shock concept elicits a statistically significant degree of wage-stickiness for these GNP-components.

Table 4-32 gives F-statistics testing the null hypothesis that the pattern of wage-stickiness revealed for a particular component of GNP is identical to the pattern of response by that category to nominal-demand shocks. The pattern is imposed over the 1946-85 sample (except for the second nonresidential-structures equation), and results are presented for the eight annual nominal-demand-shock concepts utilized throughout this study. The null hypothesis is imposed via the general procedure outlined in the discussion accompanying Hypothesis 13 (Chapter 3, pp. 170-173). All positively-signed shock coefficients with t-statistics of 1.5 or larger were taken for purposes of imposing the pattern tests, while all other shock coefficients were set to zero. Findings indicate fairly robust evidence that the persistence of nonresidential structures can be attributed to wage-stickiness, but little evidence that the response to shocks of any of the other GNP-components analyzed can be explained by the Fischer mechanism.

Columns 1 through 4 of Table 4-32 give the results for nonresidential structures. Little can be learned from the first two columns due to the general lack of explanatory power all shocks have in a nonresidential-structures equation estimated over the 1946-85 sample. However, columns 3 and 4 indicate a substantial ability for the pattern of wage-stickiness to account for the persistence of this category over the 1954-85 sample. In the unrestricted nonresidential-structures equations estimated over this sample, F-tests for the joint significance of all lagged shocks reject the null hypothesis fairly robustly across

shock concepts (Table 4-9, pp. 238-241 above). Four of eight nominal-demand-shock concepts generate a rejection of the null hypothesis where the dependent variable is the GNP-weighted ratio of STR to Y, and five of eight generate a rejection where the dependent variable is L(STR). However, in columns 3 and 4 of Table 4-32, only the two F-tests where DMRM is the shock concept reject the null hypothesis that the pattern of wage-stickiness accounts for all persistence in STR over the 1954-85 sample. Further, the two Fs that remain significant drop substantially in value (as a comparison with Table 4-9 will show), and are now significant only at the five percent level. Thus, in the annual data there is evidence that the pattern of wage-stickiness in nonresidential construction can account for the pattern of persistence exhibited by that GNP-coefficient in response to nominal-demand shocks.

Such evidence is not forthcoming for the other three GNP-components, however. Columns 5 and 6 give results where the dependent-variable concept is some function of residential construction (H). It will be recalled that H did not exhibit a persistent response to nominal-demand shocks in the annual data, and that this finding was consistent with the time-to-build hypothesis. Further, it has been seen in Tables 3-30 and 3-31 that there is fairly robust evidence of wage-stickiness in the residential-construction industry. Results from Table 3-32 indicate that too much wage-stickiness is exhibited by residential construction for that pattern to explain statistically the (lack of a) response to lagged shocks by this

category. Most of the rejections, however, come about in column 5, where the dependent variable is (H/Y)·L(Y); where the dependent variable is L(H), only one case out of eight rejects the null hypothesis. Thus results are somewhat mixed for this category.

Columns 7 and 8 give the results for producers' durable expenditures (PDUR), which it will be recalled robustly responds to shocks with the most persistence of any GNP-component. Recalling that no statistically significant wage-stickiness was observed for PDUR, it is not surprising to find the pattern test rejecting the null hypothesis robustly across all shock concepts. Finally, results for consumer perishables (CPER, in columns 9 and 10) are roughly consistent with the hypothesis. For this category results in column 9 should be heavily discounted, since there is fairly robust evidence (for example, in Table 4-6, or in a comparison of the results from Table 4-9 for this category with those of Table 4-10) that lagged shocks cause the GNP-weighted ratio of CPER to Y to fall. However, column 10 indicates little evidence that the pattern of wage-stickiness differs from the pattern of lagged shock coefficients in the L(CPER) equation. Since neither wage-stickiness nor persistence is observed for this category, this result is consistent with the wage-stickiness mechanism.

In sum, analysis of the annual data reveals some evidence that wage-stickiness plays a role in generating a persistent response by nonresidential construction to nominal-demand shocks (over the 1954-85 period). Other results, however, do not

support the view that the wage-stickiness mechanism plays a major role in generating the persistence of other components of GNP. In particular, the failure of the wage-stickiness mechanism to account for the very strong and robust persistence exhibited by producers' durable expenditures is a problem for explanations of GNP-persistence based on this mechanism.

Quarterly Results

Quarterly analysis is carried out over the 1954:I-1979:III sample for the four components of real GNP analyzed above, as well as for consumer services, the behavior of this last being investigated over a 1964:III-1979:III sample (hourly-earnings data for the consumer-services industries begins in 1964). Tables 3-33 through 3-36 present the results. Findings do not support the hypothesis that the persistence exhibited by the components of GNP in the quarterly data is due to wage-stickiness.

A potentially serious problem for the quarterly work on wage-stickiness involves the issue of seasonal adjustment of the various series. While all of the price indexes utilized in this section are seasonally adjusted, average hourly-earnings data for durable- and nondurable-goods manufacturing are not available in seasonally-adjusted form (nor are the various price indexes available in nonseasonally-adjusted form). This means that, in forming the measure of wage-stickiness (d[L(P)]-d[L(W)]), in the cases of producers' durable expenditures and consumers' perishable-goods expenditures the measure is formed by taking the seasonally-adjusted data for price and pairing it with the

nonseasonally-adjusted data for average hourly earnings. Such a procedure implicitly assumes that there is relatively little seasonal movement in the hourly-earnings data, a not unreasonable assumption if one presumes that the bulk of hourly earnings typically is composed of "straight-time" wages, which should not have a substantial seasonal component. The problem does not arise for the other series, for which seasonally-adjusted data are available both for price and for hourly earnings.

Barring special problems stemming from the seasonal-adjustment issue, the interpretation of quarterly findings is straightforward. Table 4-33 presents the wage-stickiness equations (which, again, are actually measures of wage-stickiness relative to price-stickiness) for the five types of output considered, where Barro-type money shocks are used as shock concept. Results indicate a general absence of a statistically significant amount of wage-stickiness, as, for all equations, the F-test for joint significance of all lagged shocks fails to reject the null hypothesis of no joint significance. Inspection of individual coefficients reveals some evidence of wage-stickiness in nonresidential construction (as in the annual data), but little meaningful evidence for the other sectors.

Table 4-34 investigates the robustness of the wage-stickiness findings derived using Barro-type shocks to a variation in shock concept. Results are somewhat more favorable to the wage-stickiness hypothesis when other shock concepts are used as explanatory variables. For nonresidential construction, three out of seven of the nominal-demand-shock concepts generate

TABLE 4-33: QUARTERLY "WAGE-STICKINESS" EQUATIONS FOR FIVE TYPES OF OUTPUT, USING BARRO-TYPE MEASURES OF UNANTICIPATED MONEY GROWTH, 1954:I-1979:III U.S. DATA

Explanatory Variable	(1) P-WCONN	(2) P-WCONR	(3) P-WDUR	(4) P-WNDUR	(5) P-WSERV
Constant	0.20	0.13	0.02	0.08	-0.02
	(3.3)	(2.0)	(0.3)	(1.5)	(-0.6)
t	0.0001	0.0002	-0.00001	0.00005	0.0002
	(3.2)	(4.5)	(-0.2)	(1.3)	(4.0)
LFG	-0.05	-0.04	-0.004	-0.02	0.0003
	(-3.5)	(-2.3)	(-0.3)	(-1.6)	(0.04)
DMR	0.52	0.37	-0.29	0.06	-0.04
	(2.4)	(1.3)	(-1.3)	(0.3)	(-0.3)
DMR1	0.40	0.29	-0.07	0.03	0.19
	(1.7)	(1.0)	(-0.3)	(0.1)	(1.2)
DMR2	0.55	0.39	-0.55	0.12	-0.23
	(2.3)	(1.4)	(-2.3)	(0.5)	(-1.4)
DMR3	0.25	0.11	-0.33	-0.13	-0.36
	(1.0)	(0.4)	(-1.4)	(-0.6)	(-2.2)
DMR4	0.61	0.30	-0.03	0.49	0.17
	(2.5)	(1.0)	(-0.1)	(2.3)	(1.0)
DMR5	0.48	0.09	0.11	0.20	0.20
	(2.0)	(0.3)	(0.5)	(0.9)	(1.2)
DMR6	0.41	0.40	-0.03	0.36	-0.56
	(1.7)	(1.4)	(-0.1)	(1.7)	(-3.4)
DMR7	-0.18	-0.40	-0.33	0.12	-0.40
	(-0.7)	(-1.4)	(-1.4)	(0.6)	(-2.5)
DMR8	0.37	0.10	0.39	0.49	0.23
	(1.6)	(0.4)	(1.7)	(2.4)	(1.4)
DMR9	0.13	-0.38	0.36	-0.01	0.33
	(0.6)	(-1.3)	(1.6)	(-0.05)	(2.1)
DMR10	-0.24	-0.25	-0.03	-0.05	-0.001
	(-1.1)	(-0.9)	(-0.1)	(-0.3)	(-0.004)
DMR11	0.02	0.001	0.15	0.15	-0.28
	(0.1)	(0.002)	(0.7)	(0.8)	(-1.9)
DMR12	-0.48	-0.10	0.05	0.58	0.17
	(-2.2)	(-0.4)	(0.2)	(2.9)	(1.1)
DMR13	0.12	0.04	0.19	-0.12	0.28
	(0.5)	(0.1)	(0.9)	(-0.6)	(1.8)
DMR14	-0.04	0.23	-0.22	-0.26	-0.38
	(-0.2)	(0.8)	(-1.0)	(-1.3)	(-2.4)
DMR15	0.08	0.04	0.36	-0.34	-0.21
	(0.4)	(0.1)	(1.6)	(-1.7)	(-1.4)
DMR16	0.05	0.10	0.56	-0.14	0.04
	(0.2)	(0.3)	(2.6)	(-0.7)	(0.3)

TABLE 4-33--continued

Explanatory Variable	(1) P-WCONN	(2) P-WCONR	(3) P-WDUR	(4) P-WNDUR	(5) P-WSERV
a1	0.27 (2.4)		0.23 (2.0)		
a2					
a3		0.19 (1.6)			
a4					
a5			-0.18 (-1.6)		
a6					
a7					
a8	-0.19 (-1.6)				
R^2	0.54	0.41	0.40	0.38	0.76
s	0.01	0.01	0.01	0.01	0.01
Q (6 lags)	0.41	0.07	0.10	0.18	1.54
Q (24 lags)	14.08	15.86	10.08	16.07	26.64
$F(n,d)^a$	1.68	0.85	1.49	1.75	1.19

Notes: See Table 4-1 for definitions of variables. t-statistics for coefficients are in parentheses immediately below the coefficient. ai is the ith autocorrelation coefficient, s is the standard error of the regression, and Q is the Q-statistic used to test for residual randomness. Only values for ai with t-statistics equal to 1.5 or larger are reported.

Sample periods begin in 1954:I for all equations except SERV, for which sample begins in 1964:III. All samples end in 1979:III.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

[a]F-statistic is formed by restricting to zero the coefficients of all "relevant" lagged DMRs, where "relevant" is defined as specified on pp. 226-228, above. F(16,75) (F(16,34)

TABLE 4-33--continued

for SERV) holds for all equations except the following (for which F-test degrees of freedom follow): P-WCONN, F(15,75); P-WDUR, F(15,75), P-WSERV, F(12,34). The corresponding F-statistics for these cases where all lagged DMRs are restricted to zero are reported in Table 4-35.

TABLE 4-34: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL "RELEVANT" LAGGED NOMINAL-DEMAND SHOCKS EQUAL ZERO, QUARTERLY "WAGE-STICKINESS" EQUATIONS, SEVEN SHOCK CONCEPTS, VARIOUS PERIODS, 1954:I-1979:III U.S. DATA

Shock Concept	(1) P-WCONN	(2) P-WCONR	(3) P-WDUR	(4) P-WNDUR	(5) P-WSERV
DMR	(1.68)	0.85	(1.49)	1.75	(1.19)
DMRG	2.35**	1.04	1.33	(1.20)	(0.48)
DMRM	2.41**	1.49	2.33**	1.76	(0.86)
DMRN	(1.06)	0.79	(1.23)	(2.46)**	(1.67)
DBR	2.93**	(0.67)	1.29	2.03*	1.10
DYRG	(0.56)	0.88	(3.10)**	1.25	(1.72)
DYRM	0.73	(0.61)	4.27**	(1.27)	(1.57)

Notes: See Table 4-1 for definitions of variables. Sample periods for all P-WSERV equations begin in 1963:III, and all samples for equations other than P-WSERV which use the DMRN shocks as explanatory variables begin in 1958:I. Samples for all others begin in 1954:I. All samples end in 1979:III.

For the definition of "relevant" in the present context, see the discussion on pp. 226-228, above. F-statistics in parentheses leave one or more lagged shocks unrestricted; others restrict all lagged shocks to zero. Following is a list of: (a) F-statistic degrees of freedom, and, (b) lagged shocks left unrestricted, for all Fs in parentheses (presented in the general form $F(n,d)\ \{\cdot\}$, where within brackets are the numbers corresponding to the DMR lags left unrestricted). That is, the first entry below, for DMR and P-WCONN, means: F degrees of freedom are 15 in numerator and 75 in denominator, and DMR12 is left unrestricted (since its coefficient was both "wrongly-signed" and significant at the five percent level).

For DMR: P-WCONN F(15,75),{12}; P-WDUR F(15,75),{2}; P-WSERV F(12,34),{3,6,7,14}. For DMRG: P-WNDUR F(15,75),{13}; P-WSERV F(15,34),{6}. For DMRM: P-WSERV F(15,34),{6}. For DMRN: P-WCONN F(15,59),{12}; P-WDUR F(14,59),{2,7}; P-WNDUR F(15,59),{15}; P-WSERV F(12,34),{3,6,7,11}. For DBR: P-WCONR F(15,75),{7}. For DYRG: P-WCONN F(14,75),{7,10}; P-WDUR F(15,75),{12}; P-WSERV F(15,34),{13}. For DYRM: P-WCONR F(15,75),{10}; P-WNDUR F(15,75),{8}; P-WSERV F(15,34),{13}.

F-statistics corresponding to the above which restrict all 16 lagged shocks to zero are presented in Table 4-35.

TABLE 4-34--continued

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-35: F-STATISTICS TESTING NULL HYPOTHESIS THAT ALL LAGGED NOMINAL-DEMAND SHOCKS EQUAL ZERO, QUARTERLY "WAGE-STICKINESS" EQUATIONS, EIGHT SHOCK CONCEPTS, VARIOUS PERIODS, 1954:I-1979:III U.S. DATA

Shock Concept	(1) P-WCONN	(2) P-WCONR	(3) P-WDUR	(4) P-WNDUR	(5) P-WSERV
DMR	2.06*		1.87*		2.02*
DMRG				1.26	0.63
DMRM					0.88
DMRN	1.40		1.67	2.69**	1.96*
DBR		1.27			
DYRG	0.70		3.09**		2.03*
DYRM		1.09		1.57	2.10*

Notes: See Table 4-1 for definitions of variables. F-statistics are formed by restricting all lagged shocks to equal zero (omitted Fs are reported in Table 4-34). Sample periods for all P-WSERV equations begin in 1963:III, and all samples for equations other than P-WSERV which use the DMRN shocks as explanatory variables begin in 1958:I. Samples for all others begin in 1954:I. All samples end in 1979:III.

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

TABLE 4-36: F-STATISTICS TESTING NULL HYPOTHESIS THAT THE PATTERN OF RESPONSE OF X TO ALL CURRENT AND LAGGED NOMINAL-DEMAND SHOCKS IS IDENTICAL TO THE PATTERN OF "WAGE STICKINESS," SEVEN SHOCK CONCEPTS, VARIOUS PERIODS, 1954:I-1979:II QUARTERLY U.S. DATA (WHERE X IS SOME COMPONENT OF GNP)

Shock Concept	STR		H		PDUR	
	(1)	(2)	(3)	(4)	(5)	(6)
DMR	1.61	2.19*	2.02*	1.91*	2.21*	2.80**
DMRG	0.61	0.96	1.85*	1.74	2.52**	2.66**
DMRM	0.95	1.47	3.20**	3.42**	3.38**	3.33**
DMRN	2.30**	2.91**	2.16*	2.08*	2.51**	3.66**
DBR	1.55	1.76	1.16	1.03	1.13	1.74
DYRG	1.09	1.28	0.98	1.34	1.12	1.73
DYRM	1.37	1.38	1.31	1.36	1.73	2.34**

TABLE 4-36--continued

Shock Concept	CPER		CSERV	
	(7)	(8)	(9)	(10)
DMR	0.69	3.99**	1.07	10.90**
DMRG	1.12	1.18	1.39	5.21**
DMRM	1.52	0.65	1.77	1.63
DMRN	0.71	2.63**	1.28	5.15**
DBR	1.11	1.16	1.27	4.19**
DYRG	3.23**	1.87*	4.00**	7.57**
DYRM	4.03**	1.58	3.82**	3.02**

Notes: See Table 4-1 for definitions of variables. Sample

TABLE 4-36--continued

periods for all CSERV equations begin in 1963:III, and samples for equations other than CSERV which use the DMRN shocks as explanatory variables begin in 1958:I. Samples for all others begin in 1954:I. All samples end in 1979:III.

For all F-statistics, the left-hand-side Fs are those from a regression where the dependent variable is of the general form $(X/Y)\cdot L(Y)$, and the right-hand-side Fs are those from a regression where the dependent variable is of the general form $L(X)$, where, in both cases, X is the component of GNP indicated.

Where a particular pattern of predicted shock effect can be deduced from the "wage-stickiness" equations, F-statistics have the following degrees of freedom: all CSERV equations, F(16,35); all equations using DMRN except CSERV, F(16,59); all others, F(16,75).

Where the "wage-stickiness" equations fail to indicate a statistically significant impact of shocks, F-statistic degrees of freedom are as described in the preceding paragraph except that numerator degrees of freedom equal 17 rather than 16 (since all lagged shocks and the contemporaneous shock are restricted to zero in such a case).

*Estimate is statistically significant at the five percent level.

**Estimate is statistically significant at the one percent level.

F-statistics which reject the null hypothesis that all "relevant" lagged shocks have zero coefficients. Interestingly, results for quarterly data do not confirm the previous finding for annual data that residential construction exhibits the most wage-stickiness of any category: No evidence of wage-stickiness can be found in the quarterly data for residential construction. There is some evidence of wage-stickiness in manufacturing when quarterly data are used. For producers' durable expenditures, three of seven nominal-demand-shock concepts generate a rejection of the null hypothesis that all "relevant" lagged shocks have zero coefficients (and all at the one percent significance level). For consumers' perishable expenditures, two of seven reject this hypothesis for quarterly data. Both these findings are at variance with the annual results, which find no evidence of wage-stickiness for these categories. Moreover, inspection of the individual equations corresponding to these rejections of the null hypothesis (not included in this study) reveal no systematic pattern of significant coefficients that can be easily interpreted: One would not predict a significant F-statistic on the basis of an inspection of these coefficients and their properties. Finally, no evidence of a statistically significant degree of wage-stickiness is evident in the consumer-services equations.

Table 4-36 presents F-statistics derived by imposing on the five GNP-components the null hypothesis that the pattern of shock coefficients generated in the appropriate wage-stickiness equation is identical to the pattern of shock coefficients in the

unrestricted GNP-component equations. As for the annual work, all positive shock coefficients with t-statistics of 1.5 or greater were taken for purposes of imposing a pattern, while coefficients on all other shocks were set equal to zero. The results are easily summarized: As a comparison with Table 4-14 will show, there is no evidence that the pattern of wage-stickiness helps to explain the pattern of persistence exhibited by the five GNP-components analyzed. Rejection of the null hypothesis is highly robust for all those cases where the unrestricted regression revealed a statistically significant role for lagged shocks in explaining variations in the dependent variable. Accordingly, the wage-stickiness mechanism does not account for the persistent response of the major components of GNP in the quarterly data.

In sum, results both for annual and quarterly data reveal intermittent evidence of wage-stickiness for several of the components of GNP. However, to the extent that these components exhibit a persistent response to nominal-demand shocks, there is little evidence that the pattern of wage-stickiness can account for the persistence of these several categories (some such evidence is found for nonresidential construction in the annual data). Accordingly, evidence stemming both from the annual and from the quarterly research does not support Hypothesis 13 and the proposition that the wage-stickiness propagation mechanism of Fischer (1977a) plays a major role in generating the persistent response of real GNP to measures of nominal-demand shocks.

CHAPTER 5
SUMMARY AND CONCLUSIONS

Summary

The persistent response of real macroeconomic variables to measures of unanticipated nominal-demand growth poses a challenge to the Rational Expectations approach to monetary theory. It is not immediately clear why such variables should be affected by past mistaken expectations in a world characterized by Rational Expectations. While several theoretical explanations of such persistence have been developed which are consistent with the hypothesis of Rational Expectations, little empirical evidence exists regarding the extent to which these several "propagation mechanisms" are consistent with the historical record.

This study is an extensive empirical investigation of one prominent propagation mechanism: the "time-to-build" mechanism of Kydland and Prescott (1982) (adjusted to emphasize the potential impact of nominal-demand shocks). The basic methods of approach are two: first, to disaggregate GNP by type of product and investigate the persistence of the components of GNP under various conditions; and, second, to investigate the persistence of various macroeconomic variables closely related to but not actually part of GNP (such as inventory stocks and decisions by firms to start multiperiod construction projects). In developing and carrying out empirical testing of the time-to-build

mechanism, two main classes of tests are utilized. First, the question is assessed of whether the predictions of the time-to-build mechanism are consistent with the historical record--whether the predictions of that mechanism are statistically rejected by the data. Second, the explanatory power of the time-to-build mechanism is directly tested against the explanatory power of two of the leading alternative propagation mechanisms which have been advanced in the literature; specifically, the "inventories-based" mechanism of Blinder and Fischer (1981), and the "wage-stickiness" mechanism of Fischer (1977a).

Testing is carried out through use of Barro's "unanticipated money growth" technique, for both annual and quarterly U.S. postwar data. Throughout the study attention is given to the question of the robustness of results to a variation in the method used to measure nominal-demand shocks, and to the question of the robustness of results to a change from "two-step" to "joint" estimation techniques. Within this framework a number of specific issues are addressed.

Results Stemming from the Disaggregation of Real GNP

First, the persistence of the components of GNP is assessed from the perspective of whether the various patterns of persistence revealed by these components are consistent with the predictions of the time-to-build hypothesis. Results on the whole are consistent with the time-to-build hypothesis. Evidence from the disaggregated GNP equations indicates that "strong persistence" (the tendency of lagged shocks to cause an output-category to rise relative to GNP) is confined to the investment

accounts within GNP. Some evidence of "weak persistence" (the tendency of lagged shocks to cause the log-level of an output-category to rise) is observed also in the consumption accounts. No evidence of persistence is observed for either government expenditures (not only total but also federal and state-and-local) or net exports. The combined evidence for investment and consumption suggests that shocks first impact directly on investment, and that this change in investment then brings about a further change in consumption (perhaps through a Keynesian-type multiplier effect). Thus the evidence stemming from the analysis of the disaggregated components of GNP suggests that, whatever its exact nature, the key to the process propagating nominal-demand shocks lies in the investment accounts. Since the time-to-build mechanism is an investment-accounts-based mechanism, this finding is favorable to time-to-build.

Further evidence regarding the consistency of the time-to-build mechanism with the data is acquired by disaggregating the investment accounts within GNP and investigating the patterns of persistence associated with these several categories. Results indicate that the bulk of the "strong persistence" exhibited by fixed investment is to be found in producers' durable expenditures. Residential construction exhibits short-term "strong persistence" in the quarterly data and no persistence in the annual data (although in the annual case the contemporaneous shock term is statistically significant), results thoroughly consistent with the time-to-build mechanism. An important deviation from the predictions of the time-to-build hypothesis is

the relatively small amounts of persistence exhibited by nonresidential structures. In the annual data, over a 1946-85 sample nonresidential structures exhibits no persistence in response to nominal-demand shocks. When the annual sample is shortened to 1954-85 (to allow for likely structural changes due to tax-related factors in the early 1950s), considerable "strong persistence" is exhibited by nonresidential structures. However, over a 1954:I-1979:III quarterly sample, nonresidential structures exhibits little persistence even given the exclusion of the pre-1954 data (or, for that matter, excluding pre-1960 data). The overall failure of nonresidential structures to respond in accordance with the predictions of the time-to-build mechanism raises important questions about the viability of that mechanism as an explanation of the persistent impact of nominal-demand shocks on real variables. However, if the 1954-85 annual results can be taken as representative of the response of nonresidential structures to such shocks, then results stemming from the disaggregation of fixed investment are completely consistent with the time-to-build hypothesis.

Further Results Stemming from the Disaggregation of Real GNP: Blinder-Fischer versus Time-To-Build

The finding that "strong persistence" is confined to the investment accounts within GNP is consistent with time-to-build, but it also is consistent with the inventories-based Blinder-Fischer hypothesis (another investment-accounts-based propagation mechanism). Using the disaggregated GNP-accounts data, a second series of tests is carried out in order to compare the

explanatory power of the time-to-build mechanism with that of the Blinder-Fischer mechanism. First, the persistence of changes-in-inventories (actually, the ratio of such changes to GNP) is investigated. Results indicate that lagged nominal-demand shocks generate a positive and statistically significant effect on the ratio of inventory-changes to GNP, so that this category also exhibits "strong persistence" in response to such shocks. An important necessary condition in order for the Blinder-Fischer mechanism to account for GNP-persistence is thus satisfied. Second, the explanatory power of the time-to-build hypothesis is pitted directly against the alternative, inventories-based (Blinder-Fischer) hypothesis. The null hypothesis that the pattern of persistence exhibited by GNP is identical to that exhibited by fixed investment (predicted by time-to-build) is tested against the alternative hypothesis that the pattern of persistence exhibited by GNP is identical to that exhibited by changes-in-inventories (predicted by Blinder-Fischer). In the annual work, strong results emerge from this test in favor of time-to-build: The data clearly cannot distinguish between the persistence of GNP and that of fixed investment, while the data soundly reject the hypothesis that all GNP-persistence is accounted for by changes-in-inventories. In the quarterly work, the data marginally fail to reject the Blinder-Fischer version of the hypothesis and marginally reject the time-to-build-based version. Thus, while these results are somewhat mixed, on balance they favor time-to-build.

Tests of Time-To-Build Using Independent Survey Data on Production Periods (and Related Tests)

The above results indicate that the time-to-build propagation mechanism is, on balance, consistent with the historical record captured in the U.S. GNP accounts, but stronger tests are required if an accurate picture of that mechanism's explanatory power is to be arrived at. A third series of tests is carried out with the objective of addressing this deficiency. The time-to-build mechanism predicts that the persistent impact of nominal-demand shocks on real GNP and its components ultimately is the result of the fact that many types of investment goods have production periods of considerable length. By measuring the average construction progress patterns for such projects and by imposing the restriction that the pattern of persistence in the relevant investment equation is identical to the pattern of such progress, a definitive test of the time-to-build hypothesis is constructed. From U.S. Department of Commerce survey data, estimates of such progress patterns are derived for residential and nonresidential structures, and the test described above is carried out for these two investment categories (for quarterly data). The explanatory power of the resulting specification is assessed, first, against that of the unrestricted equation for each investment category, and, second, against that of the alternative hypothesis that the pattern of persistence exhibited by investor decisions to "start" projects falling into this category is identical to the pattern exhibited by the unrestricted equation. The meaningful interpretation of results for nonresidential construction is hampered by the

general failure of that category to exhibit persistence in the quarterly data; nonetheless, the overall conclusion emerging is that the quarter-by-quarter average progress pattern for the construction of nonresidential structures does not successfully account for the persistence that is exhibited by the unrestricted nonresidential-structures equation. A similar finding holds in the case of residential structures. Further, in both these cases the specification resulting from imposing the "time-to-build" pattern on the model does not outperform the specification where the "starts" pattern is imposed on the model (indeed, for residential structures the "starts" pattern appears capable of completely explaining the persistence of this investment category). These results indicate--contrary to the prediction of the Kydland-Prescott time-to-build mechanism--that allowing for the fact that structures typically take considerable "time to build" is not sufficient to explain the persistence of this category (and by implication, is also insufficient to explain the persistence of real GNP).

It is also apparent that the length of average production periods within the category of producers' durable equipment cannot account for the very persistent response exhibited by this GNP-component in response to nominal-demand shocks. While detailed survey data on average production periods within this category do not exist, sufficient information is available to confidently deduce that average production periods for this category are considerably less than a year in duration. Thus, if the length of production periods within a category determines (on

time-to-build premises) the extent of the persistence that should be displayed by a category, then producer durables should not exhibit persistence in excess of a year in duration. Statistical testing of the hypothesis that producers' durable expenditures do not exhibit persistence greater than a year in duration leads to a strong rejection of this null hypothesis. Thus, on the above premises the very persistent response of producers' durable expenditures represents, not an affirmation, but rather a contradiction of the time-to-build hypothesis.

There is, however, a second possible time-to-build-based explanation of the persistence of producers' durable expenditures which also deserves examination. Given the complementary relationship prevailing between nonresidential structures and the equipment needed to fill those structures, it is reasonable to presume that businesses time the ordering of their equipment so that it arrives at about the time of the completion of the new structure in which the equipment is to be placed. This line of reasoning suggests that the average completion pattern for nonresidential structures might account for the persistence of producers' durable expenditures. U.S. Commerce Department survey data exist giving the number of months from start to completion of nonresidential structures, and from these data an average start-to-completion pattern for nonresidential structures is constructed. The null hypothesis is then investigated (for quarterly data) that the pattern of persistence exhibited by producers' durable expenditures is identical to the average start-to-completion pattern for nonresidential structures. In

general, the data are unable to reject this hypothesis; moreover, the hypothesis fares much better than the alternative hypothesis that the persistence-pattern of producer durables is identical to that of business decisions to start (to order the production of) producer durables. Thus, while there is no evidence that average production periods within producers' durable equipment can account for the persistence of this category, there is persuasive evidence that average production periods for nonresidential structures can account for producer-durable persistence. Since the bulk of the persistence exhibited by fixed investment is to be found in producers' durable expenditures, the above result implies that the bulk of fixed-investment persistence is explainable in terms of the time-to-build hypothesis. However, the importance of this result can be questioned due to the failure of nonresidential structures to unambiguously exhibit persistence.

The issue of producer-durable persistence seems sufficiently important to warrant further analysis. Accordingly, producers' durable equipment is disaggregated by type of product and the persistence of its several components is investigated. Results are consistent with the above version of the time-to-build mechanism: Those categories most likely to be associated with industrial structures (industrial equipment) exhibit the most persistence, while those categories least likely to be associated with nonresidential structures (autos) exhibit substantially less persistence. These findings lend some additional support to the results discussed in the previous paragraph.

Analysis of Decisions to Start Multiperiod Investment Projects

The time-to-build propagation mechanism does not predict that investor decisions to start multiperiod investment projects ("starts") should exhibit substantial persistence in response to nominal-demand shocks. Lengthy production periods are not typically associated with "starts"; further, while, given a positive nominal-demand shock in period t, it is easy to see why a rational investor would plan in t to start a project in period t+j, it is not clear why such an investor should choose to go ahead with his plans in t+j assuming that he has full information about events in t. Thus an investigation of the persistence of "starts" constitutes a decisive test of the time-to-build mechanism.

On the basis of the above reasoning, a fourth series of tests (using quarterly data) is carried out with the objective of investigating the response of "starts" to nominal-demand shocks. Results indicate that, in several important cases, investor decisions to start multiperiod investment projects exhibit far too much persistence to be reconcilable with the "pure" Kydland-Prescott time-to-build mechanism. Particularly notable in this regard are the extensive patterns of persistence displayed by "construction contracts awarded for commercial and industrial buildings" and by "manufacturers' evaluation of their plant and equipment facilities: percent saying 'less needed'" (negative persistence is observed for this latter case). The inability of the time-to-build mechanism to account for the persistence of "starts" raises serious questions concerning the viability of

that mechanism as an explanation of GNP-persistence. On the other hand, results for some important "starts" measures are consistent with the time-to-build hypothesis: No persistence is exhibited either by "newly approved capital appropriations by the 1000 largest manufacturing firms," or by "manufacturers' evaluation of their plant and equipment facilities: percent saying 'more needed'." Further, the considerable persistence exhibited by orders for nondefense producer durables can be reconciled with the time-to-build mechanism (pp. 420-421, above). On the whole, however, sufficient persistence is exhibited by "starts" to raise questions about the viability of the "pure," Kydland-Prescott type of time-to-build mechanism examined in this study.

Results Stemming from the Disaggregation of Real Inventory Stocks

Previous studies investigating the persistence of real inventory stocks have done so on the premise that the persistent response by such stocks to nominal-demand shocks constitutes evidence favoring the Blinder-Fischer propagation mechanism. However, the persistence of inventories also can be accounted for by the time-to-build propagation mechanism. A positive nominal-demand shock brings about more production of producers' durable equipment. Some such equipment takes considerable time to build (a quarter or more), while much (most?) such equipment is destined to be placed in nonresidential structures currently under construction. In the interval while such equipment is under construction, it is counted in the National Income and Product Accounts as work-in-progress inventories, implying that

total inventories should exhibit persistence due to time-to-build. Because both time-to-build and Blinder-Fischer imply that inventories should exhibit a persistent response to nominal-demand shocks, a natural theater in which to investigate the relative explanatory powers of the time-to-build hypothesis versus the Blinder-Fischer hypothesis is the behavior of inventory stocks over time. The stronger the case favoring the hypothesis that time-to-build plays a major role in bringing about any persistence exhibited by inventory stocks, the weaker the case favoring the Blinder-Fischer mechanism as an empirically significant hypothesis, and vice versa. A fifth series of tests carried out in this study is conducted along these lines (using quarterly data).

As an initial step, the persistence of total inventory stocks is investigated. Previous studies (Horaf, 1983, Demery and Duck, 1984) had found evidence of negative short-term persistence by such stocks, but had not found evidence of the longer-term positive persistence that is a necessary condition of both the time-to-build and inventories-based propagation mechanisms as applied to inventories. The present study, by contrast, finds extensive evidence of long-term positive persistence, but no evidence of short-term negative persistence. The absence of short-term negative persistence extends to the analysis of the disaggregated components of real inventory stocks. Since such a short-term response is predicted by the Blinder-Fischer mechanism but not the time-to-build mechanism,

this result favors the time-to-build-based explanation of the persistence of inventories.

Further analysis of the persistence of real inventory stocks is carried out by disaggregating these stocks and analyzing the persistence of the components of total inventories. Two key ideas constitute the basis for the investigation. First, to the extent that time-to-build is the key to explaining the persistence of total inventories, such persistence should be concentrated in manufacturers' inventories, particularly within work-in-progress inventories. By contrast, to the extent that Blinder-Fischer is the key to explaining the persistence of total inventories, such persistence should be concentrated in middleman inventories, particularly within retailers' inventories. Second, to the extent that time-to-build is the key to explaining the persistence of total inventories, inventory persistence should be concentrated in the durable components of the various inventory categories (since virtually all goods with lengthy production periods should be found within the durables category). Conversely, the Blinder-Fischer mechanism does not predict that durable stocks will respond differently to shocks than do nondurable stocks. Investigation of these two pairs of conflicting hypotheses reveals that neither hypothesis can be rejected by the data. First, the bulk of persistence is concentrated in only two categories: work-in-progress stocks and retailers' stocks (however, the presence of serial correlation in the residuals of the work-in-progress equations undermines somewhat the conclusions reached for this category). Second,

persistence in manufacturing stocks (including work-in-progress stocks) is confined to the durable portion of that category, while persistence in retailers' stocks is spread more or less equally across the durable and nondurable components of these stocks.

These findings suggest that there are two explanations--not a single explanation--of the persistence of total inventory stocks. Not only is it the case that, as is commonly presumed, the Blinder-Fischer mechanism generates persistence in total stocks, but it also seems probable that the time-to-build mechanism plays a prominent role in generating such persistence. Accordingly, the persistence of total inventories cannot be taken as unambiguous evidence favoring the Blinder-Fischer inventories-based explanation of GNP-persistence.

Analysis of the Wage-Stickiness Propagation Mechanism

To this point analysis of the time-to-build mechanism has involved either testing the mechanism against the data or else testing it against the inventories-based Blinder-Fischer mechanism. However, a third prominent propagation mechanism is the wage-stickiness hypothesis of Fischer, which predicts that persistence should be concentrated in those categories of GNP in which wages are most sticky relative to product prices. To the extent that substantial wage-stickiness is observed in categories where production periods are not long (or vice versa), the opportunity arises to directly test the explanatory power of the time-to-build mechanism against the wage-stickiness mechanism. Analysis is carried out for both annual and quarterly data, for

nonresidential and residential structures, producers' durable expenditures, consumers' nondurable expenditures, and (for quarterly data only) consumers' expenditures on services. Results indicate that the wage-stickiness mechanism has relatively little capacity to account for the persistence of GNP. In particular, the failure of the wage-stickiness mechanism to account for the very persistent response to nominal-demand shocks displayed by producers' durable expenditures is to be contrasted with the relative success of the time-to-build-based explanation of this persistence (pp. 420-421, above).

Conclusions and Suggestions for Future Research

The study outlined above has developed and implemented a number of empirical tests of the time-to-build propagation mechanism. While the theoretical correctness of that mechanism has been widely accepted, and while casual observation provides a wealth of evidence suggesting its empirical validity, a rigorous empirical investigation of the hypothesis has not previously been carried out. On the basis of the empirical findings summarized above, a number of general conclusions can be reached with respect to the questions of the propagation of nominal-demand shocks, and the role of the time-to-build mechanism in that propagation.

First, there is fairly broad support for the hypothesis that lengthy average production periods for many types of capital-goods are capable of accounting for a considerable amount of the persistence to nominal-demand shocks that is exhibited by real GNP. Such evidence takes several forms. First, the variations

in persistence displayed by the components of GNP are generally in harmony with what is predicted by the time-to-build hypothesis. Second, there is evidence that the pattern of persistence exhibited by real GNP is very similar to the pattern displayed by fixed investment (that component of GNP which is most susceptible to the time-to-build effect). Third, there is persuasive evidence that the average start-to-completion pattern for nonresidential structures fully explains the very persistent response to nominal-demand shocks that is displayed by producers' duroble expenditures. Since the bulk of the persistence displayed by fixed-investment expenditures is to be found within producers' durable expenditures, and since the persistence of GNP seems mostly tied up with that of fixed investment, this finding means that much of the persistence of GNP is directly explainable in terms of the time-to-build hypothesis. Fourth, there is some evidence suggesting that a good portion of the persistence of inventories can be accounted for by the time-to-build hypothesis, so that, to the extent that it can be argued that the persistence of inventories accounts for the persistence of GNP, part of that effect can be attributed to time-to-build. Fifth, the time-to-build hypothesis fares well against its two main competitors: the Blinder-Fischer mechanism ond the wage-stickiness mechanism. Thus there is a considerable body of evidence which favors some version of the time-to-build propagation mechanism as an explanotion of GNP-persistence.

At the same time, there is also evidence indicating that the simple Kydland-Prescott version of the time-to-build mechanism--

where a shock in t generates fairly rapid starts of projects which are then completed in subsequent periods even given full information in those later periods about the shock in t--cannot be regarded as a complete explanation of the persistence phenomenon. First, undercutting the case for time-to-build throughout this study is the fairly robust failure of nonresidential structures to exhibit the magnitude of persistence that is predicted by the time-to-build hypothesis. Second, the null hypothesis that the pattern of progress on nonresidential structures can account for the persistence exhibited by this category is not supported by the data. Third, the relative failure of the work with nonresidential structures, in turn, raises questions about what is arguably the most notable finding of the study--that the persistence of producer durables can be accounted for by the start-to-completion pattern of nonresidential structures. It is difficult to maintain that the persistence of nonresidential structures accounts for the persistence of producer durables when nonresidential structures does not exhibit much persistence itself. Fourth, there is no evidence that construction progress patterns can account for the persistence of residential structures; indeed, it would appear that the persistence of housing starts, rather than lengthy construction periods, is the explanation of residential-structures persistence. Fifth, there is evidence that some (though not all) investor decisions to start multiperiod construction projects respond to nominal-demand shocks with far too much persistence to be compatible with the "pure" time-to-

build hypothesis. Sixth, while there is evidence that time-to-build plays an important role in bringing about the persistence of inventories, the data also strongly suggest that the Blinder-Fischer mechanism accounts for a good portion of this persistence.

Of these six findings at variance with the time-to-build hypothesis, arguably the most significant is the persistence of "starts" (mainly "starts" of nonresidential structures). It is particularly difficult to see why a rational investor, armed with full information about the events of previous periods, would choose to "throw good money after bad" by going ahead with an as-yet unstarted project when the initial decision to engage in that project is known to have been brought about by a past nominal-demand shock. The rational option would seem to be cancellation of the project. The persistence of "starts" thus raises questions, not only about the viability of the time-to-build mechanism, but also about the viability of the general attempt to account for persistence within a framework where economic decisionmakers form expectations rationally and rapidly make use of all "available" information. It seems exceedingly difficult to account for the substantial persistence of "starts" within the framework where decisionmakers are assumed to have full information about the events of previous periods.

One may suggest a general framework within which the persistence of "starts" might be reconciled with the Rational Expectations hypothesis in combination with some aspects of the time-to-build mechanism. The mechanism would emphasize the

depreciation of capital within a basic time-to-build framework working in conjunction with a Blinder-Fischer type of mechanism. Given that a positive nominal-demand shock causes an immediate pickup in consumption demand (for which there is some evidence in Tables 4-7 and 4-12, above) and a short-term increase in production for purposes of rebuilding and/or increasing inventory stocks, it is reasonable to presume that the depreciation of capital will proceed at a more rapid rate, since available capital will be more intensively utilized in order to generate more short-term production. Then, in subsequent periods, the depreciated capital will need to be replaced, triggering an increase in investment spending for several years after the initial shock. Such a mechanism would furnish an inducement to start investment projects for some time subsequent to the date of the shock, despite full information about past events. However, the difficulty with all such mechanisms emphasizing capital-stock adjustment has been pointed out both by Lucas (1975) and Haraf (1983); specifically, it is that "cyclical variations in capital appear to be of questionable quantitative significance" (Haraf, 1983, p. 104). Still, work carried out along the lines laid out immediately above could, at least in theory, reconcile the persistence of "starts" with the Rational Expectations framework.[1]

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[1]Another possible path of reconciliation would abandon the implicit assumption that the Rational Expectations approach necessarily implies full information with a single-period lag. Lucas recently has speculated about such a path in a similar context. Discussing the workings of the Kydland-Prescott model, he states that "in imagining these elaborations, I am retaining the assumption that all information is public, but the volume of

Returning to the empirical results reported in the present study, a number of implications for future work emerge from those results. A more detailed investigation of nonresidential structures is clearly required, since the failure of this GNP-component to exhibit substantial persistence in the present study raises important questions about the viability of the time-to-build mechanism. The nature of the process that accounts for the "weak persistence" robustly displayed by consumption--assumed in this study to be a Keynesian-type multiplier triggered by an initial change in investment demand--might be more systematically investigated. The development of more reliable data on average production periods within the category of producers' durable equipment would be highly desirable in light of the considerable persistence exhibited by this category, and especially in light of the fact that the time-to-build-based explanation of inventory persistence is largely based on the assumption of lengthy (one quarter or more) average production periods for this category. A systematic reexamination of the determinants of inventory-stock persistence seems called for in light of the likelihood that time-to-build as well as Blinder-Fischer plays a role in generating that persistence (and in light of the problems with

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such information is exploding; we are now thinking of millions of bits of new information on tastes and technology becoming available each period. Each 'bit' may be easy, even free, to acquire, but with any kind of cost associated with processing this information, people are going to economize, processing only those observations that materially sharpen their ability to make their own production and investment decisions well. If we were to model this aspect of the problem, we would need to go well beyond the full-information framework Kydland and Prescott used to deal with differently informed agents. Theorists have scarcely begun to explore this territory" (Lucas, 1987, p. 97).

serial correlation plaguing the analysis of work-in-progress stocks). Most importantly, the issues raised by the robust and substantial persistence exhibited by certain "starts" concepts--most notably starts of nonresidential-construction projects--needs to be investigated in detail, from both a theoretical and an empirical perspective. Finally, any of the results reported above might be fruitfully reexamined by exploring their robustness to variation in the vector of natural-rate explanatory variables used in fitting the second-stage equation.

The time-to-build propagation mechanism is one of several possible explanations of the persistent impact of nominal-demand shocks on real variables. The evidence collected in the present study can best be summarized as being favorable towards the idea that some empirically-valid version of a time-to-build mechanism is at work in the data. However, an identification of the precise nature of that mechanism, as well as of the manner in which it interacts with other plausible propagation mechanisms, remains the task of future research.

APPENDIX A
DATA SOURCES

Virtually the whole of the data utilized in this study are taken from standard U.S. government sources. Below are the sources for all series used in this study, classified by source.

U.S. Department of Commerce

National Income and Product Accounts

The following series are taken from the National Income and Product Accounts. The specific sources are as follows: for series values up to 1982, U.S. Department of Commerce (1986a); and, for series values after 1982, U.S. Department of Commerce (1986b).

INVYS = real business inventories.

P = GNP deflator (1982=100).

Y = real GNP.

C = real consumption expenditures (CDUR+CPER+CSERV).

CDUR = real consumer expenditures on durable consumer goods.

CPER = real consumer expenditures on nondurable (perishable) consumer goods.

CSERV = real consumer expenditures on services.

GPDI = real gross private domestic investment expenditures.

DEP = real depreciation expenditures.

NPDI = real net private domestic investment expenditures.

NRFI = real nonresidential fixed investment expenditures.

PDUR = real producers' durable expenditures.

STR = real investment in nonresidential structures.

H = real investment in residential structures.

DINVY = real change in business inventories.

G = real government expenditures.

FG = real federal government expenditures.

SLG = real state and local government expenditures.

NETX = real U.S. net exports.

Data disaggregating real producers' durable expenditures by type of product:

PDINF = information processing and related equipment.

PDINFA = PDINF less "office, computing, and accounting machinery."

PDINDL = industrial equipment.

PDTRANS = transportation and related equipment.

PDTRBIG = that portion of PDTRANS composed of "aircraft, ships, boats, and railroad equipment."

PDTRSMALL = that portion of PDTRANS composed of, (a) "trucks, buses, and truck trailers," and, (b) "autos."

PDTRCKS = that portion of PDTRSMALL composed of "trucks, buses, and truck trailers."

PDAUTOS = that portion of PDTRSMALL composed of "autos."

PDELSE = other equipment (PDUR - PDINF - PDINDL - PDTRANS).

Implicit price deflators for nonresidential construction, residential construction, producers' durable goods, consumers' nondurable goods, and consumers' services.

Handbook of Cyclical Indicators

The following series are taken from U.S. Department of Commerce (1984).

ODUR = nominal value of manufacturers' new orders for durable goods (including defense-goods industries).

ONDK = nominal value of manufacturers' new orders for nondefense capital goods.

CAM = nominal value of newly-approved capital appropriations, 1000 manufacturing corporations.

OCON = construction contracts awarded for commercial and industrial buildings, square feet of floor space.

HS = new private housing units started (annual rate, thousands of units).

OH = index of new private housing units authorized by local building permits.

Survey of Current Business

The following series is from Holloway (1986).

DEBT = real value of the federal debt.

The following series are from Green and Hertzberg (1980) and Woodward (1980).

"MORE" = manufacturers' evaluation of their plant and equipment facilities: percent saying "more needed."

"LESS" = manufacturers' evaluation of their plant and equipment facilities: percent saying "less needed."

The following series are from Seskin and Sullivan (1985).

PLANONE = planned expenditures for new plant and equipment by all industries, one quarter ahead, as a percentage of actual expenditures.

PLANTWO = planned expenditures for new plant and equipment by all industries, two quarters ahead, as a percentage of actual expenditures.

Unpublished

The following series are unpublished U.S. Department of Commerce data.

FIG = real business finished-goods inventories.

FIGDU = durable-goods component of FIG.

FIGND = nondurable-goods component of FIG.

WIP = real business work-in-progress inventories.

WIPDU = durable-goods component of WIP.

WIPND = nondurable-goods component of WIP.

MAS = real business materials-and-supplies inventories.

WH = real business wholesale-trade inventories.

WHDU = durable-goods component of WH.

WHND = nondurable-goods component of WH.

RET = real business retail-trade inventories.

RETDU = durable-goods component of RET.

RETDUAUT = portion of RETDU held by auto dealers.

RETDU-AUT = RETDU - RETDUAUT.

RETND = nondurable-goods component of RET.

U.S. Department of Labor

The following series are from U.S. Department of Labor (1985, 1986).

WCONN = construction-worker average hourly earnings.

WCONR = construction-worker average hourly earnings.

WDUR = durable-goods-industry worker average hourly earnings.

WNDUR = nondurable-goods-industry worker average hourly earnings.

WSERV = service-industry worker average hourly earnings.

Citibase Data Bank

The following series are from the Citibase Data Bank (Citibank, 1986).

B = the monetary base, adjusted for reserve requirement changes.

TB = U.S. Treasury Bill rate, 3-month maturity.

Other and/or Mixed Sources

The following series are from either sources not previously cited above, or else from a mixture of sources.

M = the annual or quarterly average of the "M1" definition of the money stock. Data are from the following sources: for 1941-1946, Barro (1981b); for 1947-1969, Board of Governors of the Federal Reserve System (1976a); for 1970-1973, Board of Governors of the Federal Reserve System (1976b); for 1974-1978, Board of Governors of the Federal Reserve System (1980a); for 1979-1982, Board of Governors of the Federal Reserve System (1982); for 1983-1985, Board of Governors of the Federal Reserve System (1983-1986).

FEDV = Barro's measure of real Federal Government spending relative to normal. 1941-78 annual values are taken from Barro (1981b), while 1979-85 annual values are generated according to the formula

$$FEDV = 0.8(LFG - LFG1 + FEDV1),$$

Quarterly values for FEDV are unpublished figures supplied by Mark Rush.

U = the unemployment rate in the total labor force, including military personnel. Annual data for 1941-49 are from Barro (1981b). Annual data for 1950-85 are from U.S. Department of Commerce (1987). Quarterly values for U are from U.S. Department of Commerce (1984).

CP = Commercial Paper (prime, 4-6 months) discount rate. Data for 1946-1970 are from Board of Governors of the Federal Reserve System (1976a). Data for 1971-1984 are from U.S. Department of Commerce (1975-1984). Data for 1985 are from Board of Governors of the Federal Reserve System (1983-1986).

WAR = dummy variable set to equal zero except for the year (quarter) after the end of a war (1946, 1954, 1973), when it equals the yearly (quarterly) average of the number who had served in the military for the preceding war. Data from which variable was constructed is from U.S. Department of Commerce (1975a, 1975b).

Warning Data in this Appendix has been superseded by improved estimates These place more emphasis on larger projects (for nonres. structures) than is desirable. See Montgomery (1995) in Economics Letters, or 1998 in J. Macro, or contact the author directly for the details

APPENDIX B
GENERATION OF QUARTERLY PROGRESS PATTERNS FOR NONRESIDENTIAL AND RESIDENTIAL CONSTRUCTION

This appendix gives the details of the methods used in generating the "time-to-build" restrictions reported in Tables 3-5 and 3-10 of the text. Restrictions are developed from survey data published by the U.S. Department of Commerce. For nonresidential construction, sources are U.S. Department of Commerce (1970) and U.S. Department of Commerce (1978-1984). For residential construction, sources are U.S. Department of Commerce (1970, 1972) and U.S. Department of Commerce (1972-1986).

The raw survey data give information on monthly progress patterns for either nonresidential or residential construction projects for a sample of projects completed in the survey period. Data are broken down by project size, where for nonresidential construction size is measured by total cost of a project's construction, and for residential construction size is measured by the number of units in the structure. The raw data thus consist of a series of separate samples of construction progress patterns, where each separate sample corresponds to a different time period, and where within each time period the data on monthly progress patterns are further subdivided by approximate project size. The problem is to aggregate the samples for each output category into a summary measure of average quarterly

progress patterns over the entire sample period. The procedures adopted are described below.

Nonresidential Construction

Surveys on nonresidential construction projects are available for the following periods: 1961-65, 1976-77, 1978, 1979, 1980, and 1982-83. Two types of monthly surveys are available: surveys on value put-in-place, and surveys on the percent distribution of projects completed (measured by the number of months from start to completion). The former is used in generating the relevant portion of Table 3-5, and the latter in generating Table 3-10. Since the methods used in transforming the raw data into the tables are virtually identical for these two cases, only the generation of Table 3-5 will be discussed.

For a particular survey period, the raw data are presented in the form given in Table A-1 (which gives a portion of the 1980 value-put-in-place sample). The first step is to derive a measure of the monthly spending implied by the survey data for each size category. This involves, first, deriving a measure of the total value of construction for each size category over the sample period, and, second, distributing that total value as indicated by the progress-pattern data. The total value of construction for category i was derived as equalling the number of projects falling into category i times the average project value for that category. Average project values were assumed to equal the mean for that category for all cases except for the "5 million or more" category, for which the average project size is given by the survey (it equals $16.1 million for 1980).

TABLE A-1: PRIVATE NONRESIDENTIAL BUILDING PROJECTS COMPLETED IN 1980--PERCENT DISTRIBUTION OF VALUE PUT IN PLACE EACH MONTH FOLLOWING MONTH OF START

	Projects Costing $___ million				
_____ Month Following That of Start	5 or more	3 - 5	1 - 3	. . .	.025 - .05
0th	2.1	4.1	6.7	. . .	37.4
1st	2.4	4.8	8.3	. . .	36.4
2nd	2.7	5.9	9.7	. . .	18.5
3rd	3.4	7.0	10.2	. . .	4.1
4th	4.0	8.0	10.1	. . .	1.4
.	.	.	.		.
.	.	.	.		.
.	.	.	.		.
40th	0.1	0.0	0.0	. . .	0.0

Source: U.S. Department of Commerce (1981).

TABLE A-2: PRIVATE NONRESIDENTIAL BUILDING PROJECTS COMPLETED IN 1980--VALUE PUT IN PLACE EACH MONTH FOLLOWING MONTH OF START (MILLIONS OF DOLLARS)

	Projects Costing $___ million				
_____ Month Following That of Start	5 or more	3 - 5	1 - 3	. . .	.025 - .05
0th	229.9	80.4	180.2	. . .	4.9
1st	262.8	94.1	223.3	. . .	4.8
2nd	295.6	115.6	260.9	. . .	2.4
3rd	372.2	137.2	274.4	. . .	0.5
4th	437.9	156.8	271.7	. . .	0.2
.	.	.	.		.
.	.	.	.		.
.	.	.	.		.
40th	10.9	0.0	0.0	. . .	0.0

Note: Source of data is given in the text.

Multiplying the total value of construction for category i by each monthly progress pattern for that category yields a measure of monthly spending for category i. Results are given in Table A-2.

The second step is to move from Table A-2 to a summary measure of the 1980 average quarterly progress pattern across all size categories. This is accomplished in three substeps. First, summing across the rows of Table A-2 gives the total spending generated in each month following the month of start. Second, these monthly figures are converted to quarterly figures (months zero through two make up quarter zero, months three through five make up quarter one, etc.). Third, each quarterly figure is expressed as a ratio of the sum of all quarterly figures. The result is the quarterly average progress pattern for 1980.

The third step is to combine the result for 1980 with the results for other survey periods (derived in analogous fashion to the above) in order to derive the average quarterly value-put-in-place patterns for the entire sample. Each individual survey pattern is weighted by the ratio of the number of years covered in that survey to the number in the total sample. Thus, the 1980 figures receive a weight equal to 1/12=0.083, while the 1961-65 figures receive a weight equal to 5/12=0.417 (note there are 12, not 23, years comprising the total sample). The resulting weighted average across all the samples gives the quarter-by-quarter value-put-in-place pattern presented in Table 3-5. The same basic procedure is used in generating Table 3-10.

Residential Construction

Surveys on residential construction projects are available for the following periods: 1964-67, and each year from 1971 to 1985. However, the data give only the percent distribution of projects completed (measured by the number of months from start to completion). Transformation of these data to a period-by-period value-put-in-place series requires making assumptions about how progress on the completed projects was distributed over time (this will be discussed further below).

For a particular survey period, the raw data giving the number of months from start to completion are presented in the form given in Table A-3 (which gives a portion of the 1963-67 sample). The first step is to generate an average monthly start-to-completion pattern across all project sizes, which requires some means of weighting the contribution of each size category to the total. Data were collected on the number of buildings started in each of the size categories in the survey (the sources were U.S. Department of Commerce, 1972, and U.S. Department of Commerce, 1972-1986), and the proportion of total units started falling into the various categories was calculated. For example, over the 1963-67 period, approximately 65 percent of all units started were single-unit structures, approximately six percent of all units started were in two- to four-unit structures, etc. These figures were then used to weight the "start-to-completion" survey data, resulting in the desired average pattern for each survey.

TABLE A-3: PERCENT DISTRIBUTION OF PRIVATE RESIDENTIAL BUILDINGS COMPLETED IN 1963-67, BY NUMBER OF UNITS IN THE BUILDING AND NUMBER OF MONTHS FROM START OF CONSTRUCTION TO COMPLETION

	Buildings with ___ units				
_____ Month Following That of Start	1	2 - 4	5 - 9	. . .	greater than 50
0th	0.4	0.2	0.0	. . .	0.0
1st	3.0	1.5	0.0	. . .	0.0
2nd	12.1	7.2	2.2	. . .	0.4
3rd	27.1	16.3	5.5	. . .	3.5
4th	24.2	18.2	12.1	. . .	3.9
.	.	.	.		.
.	.	.	.		.
.	.	.	.		.
24th or greater	0.0	0.0	0.0	. . .	2.2

Source: U.S. Department of Commerce (1970).

In the second step, the first substep was to combine average patterns for the various surveys to yield one average pattern for the entire 1964-85 period. The weights used were the number of units started in each survey period, measured as a proportion of the total number of units started in all the survey periods combined. Thus, since 18 percent of the total number of units started over all survey periods were started in the 1963-67 period, the "start-to-completion" pottern for this period received a weight of 0.18 in calculating the total pattern. The second substep was to take these monthly figures and convert them into a quarterly "start-to-completion" series.

The third step is to convert the quarterly "start-to-completion" pattern for the 1964-85 period into a quarterly value-put-in-place series. To carry this out, it was assumed that, in moving from the start of projects to their completion, progress occurs at a uniform rate. For example, the final "start-to-completion" series indicates that 41.3 percent of all units completed over the survey period are completed in the first quorter after their start. Half of the progress leading to this figure is assumed to occur in the quarter of start, and the other half in the quarter after start, so that, of the 41.3 percent of units completed in the quarter after start, 20.65 percent (half of this total) is assumed to be put in place in the quarter of start, and 20.65 percent (the remaining half) in the quarter after start. The weight is 1/3 for each quorter for projects completed two quarter after start, 1/4 for those completed three

quarters after start, etc. Finally, summation of the resulting totals for period 0, period 1, period 2, etc., yields the results presented in Table 3-5.

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BIOGRAPHICAL SKETCH

Michael R. Montgomery was born in Rockledge, Florida, on November 3, 1955. His early years were spent in Cocoa Beach, Florida, where he attended Freedom 7 Elementary School, Roosevelt Junior High School, and Cocoa Beach High School. In his ninth-grade National Science Foundation research class at Roosevelt Junior High School (1969-70), under the late Larry Bechtel, he acquired a love for science which ultimately was to lead to an academic career. At Cocoa Beach High School (1970-73), Nicholas Fumero and June McDonald turned his attention to the social sciences. Mr. Montgomery attended college at Florida Southern College, Lakeland, Florida (1973-77), where he majored in English. Lectures by Larry Durrence, Ray Lott, and John Wagner, and the friendship of John Reuter, inspired him during his studies there.

In 1975 Mr. Montgomery first was exposed to economics in the Principles classes at Florida Southern of Veronica Vitelli and Merle Dimbath. In 1976, Ian Crookenden gave him a copy of Ayn Rand's novel Atlas Shrugged, which led him to tackle Benjamin M. Anderson's Economics and the Public Welfare. The result of these diverse influences was to inspire him to seek an academic career in economics.

Mr. Montgomery began graduate school at the University of Florida in 1977-78, where the early lectures of Fred Arditti and William A. Bomberger were the most memorable. He received his master's degree in 1980, and completed his coursework in 1981, with specializations in monetary theary and econometrics. He left the University to take a position in Auburn University's Department of Economics in 1983. Currently he is Visiting Assistant Professor of Economics at Auburn University.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

William A. Bomberger, Chairman
Associate Professor of Economics

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

David Denslow
Professor of Economics

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